THE

MATHEMATICAL GAZETTE

EDITED BY
W. J. GREENSTREET, M.A.
WITH THE CO-OPERATION OF
F. S. MACAULAY, M.A., D.Sc.
AND
PROF. E. T. WHITTAKER, M.A., F.R.S.

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The Mathematical Association.

THE Annual Meeting of the Mathematical Association was held at the London Day Training College, Southampton Row, London, W.C. 1, on Monday, 5th January, 1925, at 5.30 p.m., and Tuesday, 6th January, at 10 a.m. and 2.30 p.m. Professor G. H. Hardy was in the chair.

MONDAY, 5.30 *p.m.*

(1) "The Mathematical Laboratory; its scope and function," by Professor H. Levy, D.Sc., M.A., F.R.S.E.

TUESDAY MORNING, 10 *a.m.*

BUSINESS.

(2) The following Report of the Council for the year 1924 was distributed and taken as read:

DURING the year 1924, 134 new members of the Association have been elected, and the number of members now on the Roll is 1019. Of these, 8 are Honorary members, 54 are life members by composition, 24 are life members under the old rule, and 933 are ordinary members. The number of associates connected with the Local Branches is about 500.

The Council regret to have to record the death of Charles Godfrey, M.V.O., M.A. Professor Godfrey was one of the most active members of the General Teaching Committee. He served on it from its formation until the time of his death and was chairman for several years. He also served on the Council of the Association and frequently took part in the proceedings at the annual meetings. An obituary notice appeared in the *Mathematical Gazette* for July, 1924.

The Council regret to have to record the deaths also of Miss F. Exton, Principal of Lincoln Training College; Mr. W. M. Wilson, Headmaster of the Municipal Secondary School, Hanley; Mr. F. E. Jelly of King Edward VII. School, Sheffield; Professor W. J. Johnston of the University College of Wales, Aberystwyth; Mr. Reginald Gulliver of Maldon Grammar School; Miss Grace D. Sadd of University College, Exeter; Mr. Sydney Hall of Hereford Cathedral School; and the Rev. A. E. Allcock, formerly Head Master of Highgate School, who had been a member of the Association for forty-eight years.

The President (Professor G. H. Hardy) and Professor E. H. Neville were nominated by the Council to attend the Celebration of the Coming of Age of the University of Leeds and the Jubilee of its parent foundation, the Yorkshire College of Science, as representatives of the Association.

During the past year the General Teaching Committee has been considering the question of the revision of the list (published in the *Gazette*) of books suitable for a Teachers' Library. A sub-committee has been appointed to report on the possibility of re-issuing the list in a new form, making it the basis of a list of authoritative treatises suitable for a Mathematical Reference Library for Schools, excluding books used in the class-room. The Committee has also had under consideration the question of the teaching of mathematics in Technical Schools, and has appointed a sub-committee to prepare a draft report.

The Boys' Schools Committee has been investigating the subject of Experimental Work in Kinetics.

A Report on the Teaching of Mathematics in Preparatory Schools was issued to members early in the year.

Mr. W. J. Greenstreet and Rev. J. J. Milne, and others, have continued their generosity to the Library, and Professor R. W. Genese, in disposing of part of his own library, has allotted a large share to the Association. Mrs. Charles Godfrey has given, in memory of her husband, a collection of about 300 books; a book-plate with a special inscription has been inserted in each volume.

The Librarian has again been able to find private owners willing to lend books not in the Library, and almost every request made to him for the loan of a book has been fulfilled.

A catalogue of books in the Library is in the press.

During the year a branch of the Association has been formed at Melbourne, under the name of "The Mathematical Association of Victoria." The Honorary President is Professor E. J. Nanson, and the President is Professor J. H. Michell. The Honorary Secretaries are Mr. R. J. A. Barnard and Mr. J. L. Griffith.

The members present at the annual meeting will be asked to nominate and elect two members to fill the places rendered vacant

on the Council by the death of Professor C. Godfrey and the resignation of Miss E. R. Gwatkin.

The Council again desire to express the thanks of the Association to Mr. W. J. Greenstreet for his services as Editor of the *Mathematical Gazette*.

(3) The Treasurer's Report for the year 1924 was adopted.

(4) The Election of Officers and Members of the Council for the year 1925.

Mr. F. G. Hall and Mr. M. N. Gibbins were elected to fill the two places on the Council vacant in consequence of the death of Professor C. Godfrey and the resignation of Miss E. R. Gwatkin.

(5) Prof. J. E. A. Steggall delivered an address on "The Teaching of Arithmetic in Schools." Dr. W. F. Sheppard and Prof. A. Lodge took part in the subsequent discussion.

(6) Mr. Arnold Buxton read a paper on "An Application of the Bessel Functions to a Problem in Optical Resolution."

(7) It is with deep sympathy that Members will learn that the fatal illness of his father prevented Professor E. H. Neville from opening the discussion on "Tangency and Limits in Geometry." His place was taken by Dr. W. F. Sheppard, Mr. B. E. Kenworthy Brown put forward a scheme for a simple course in Geometry. Prof. M. J. M. Hill spoke on the parallelo-axiom and on similarity.

The President's remarks brought the discussion to a close.

(8) The exhibition of apparatus for use in teaching Mechanics was quite successful. The designers were Messrs. E. J. Atkinson, B.Sc., C. V. Durell, M.A., and D. F. Ferguson, M.A.

(9) The President delivered his Presidential Address, taking as his subject, "What is Geometry?"

(10) Dr. H. B. Heywood read a paper on "The Reform of University Mathematics."

The proceedings were brought to a close with the usual votes of thanks.

LOCAL BRANCH.

THE Queensland Branch held its third meeting for 1924 on the 14th November.

The following is from the Report for the year 1923-24, presented at the Annual General Meeting, held on the 28th March last:

On Friday evening, 6th April, 1923, at the University, the Second Annual General Meeting of the Society was held. The Annual Report and Balance Sheet were presented and adopted. These showed the Society to be in a very satisfactory condition.

An Election of Officers took place, and then Professor Priestley gave his presidential address on the subject, "Pure Mathematics in Relation to its Applications."

Three general meetings were held during the year.

On Friday evening, 11th May, at the Boys' Grammar School, Gregory Terrace, Mr. K. ff. Swanwick, B.A., LL.B., gave a very interesting address on "Statistics." At the conclusion of the address much discussion took place.

The second meeting was held at the University on Friday evening, 27th July. Mr. I. Waddle, M.Sc., gave a paper dealing with "Mathematics in its Relationship to Physics." The paper was very much appreciated.

It was suggested that a Dinner should be held on the 11th October, but this date did not seem to suit the members and the suggestion lapsed.

The final meeting for the year was held on Friday evening, 2nd November, at the Boys' Grammar School, Ipswich. Mr. H. J. Priest, B.A., B.Sc., gave the paper, the subject being "Greek Trigonometry." The Society was again indebted to Mr. and Mrs. Kerr for extending their hospitality to the members who visited Ipswich, thus making the trip a most enjoyable one.

During the year the usual committee meetings have been held, when all arrangements for the work of the Society were made.

Our numbers have been fairly well maintained. Last year we had in our Branch nine full members of the Mathematical Association and sixteen Associate Members.

The income of the Society was just sufficient to meet our expenses.

Associate Members have received copies of the *Mathematical Gazette* as they have come to hand.

For the coming year we have ordered two instead of three copies of the *Gazette* for the use of Associate Members, as several members who were previously Associate Members are now full members of the Mathematical Association.

Members will be interested to know that this Branch is receiving full recognition by the Mathematical Association, and a list of our members for 1922 appears in the List of Members of the Mathematical Association, June 1923.

The year has been a successful one, and it is to be hoped that this year we can increase our membership, and thus broaden our activities.

At the Annual General Meeting, held on the 28th March, 1924, the following Officers were elected:

President: Professor H. J. Priestley, M.A. (re-elected).

Vice-Presidents: Rev. Bro. Magee (re-elected); R. A. Kerr, M.A., B.Sc.

Hon. Secy. and Treas.: S. G. Brown, M.A. (re-elected).

Committee: Miss E. Cribb, M.A. (re-elected); Miss D. Lockington, B.A. (re-elected); H. J. Priest, B.A., B.Sc. (re-elected); I. Waddle, M.Sc. (re-elected); S. Stephenson, M.A.

GLEANINGS FAR AND NEAR.

290. Albertus Magnus makes [astrology] the most valuable science, because (says he) it teaches us to consider the causation of causes, in the causes of things.—(Scandal *log.*) Congreve's *Love for Love*, iii. 4.

291. Samuel Butler to his mother (written when 16), 1851:

"I have just been interrupted in this letter to prove to the whole private room, except More, that when $d=0$, $ad=0$, none of them being for some time able to comprehend the simple fact that four times nought is 0, and all insisting that 0 time 4 is a very different thing from 4 times 0!!!! and not above half believing that 5 times 6 = 6 times 5!!!! So much for the mathematical education at Shrewsbury: it really has been a most animated argument."

WHAT IS GEOMETRY?

(*Presidential Address to the Mathematical Association*, 1925.)

By Prof. G. H. Hardy, F.R.S.

I have put the title of my address in the form of a definite question, to which I propose to return an equally definite answer. I wish to make it quite plain from the beginning that there will be nothing in the least degree original, still less anything paradoxical or sensational, in my answer, which will be the orthodox answer of the professional mathematician.

I expect that you, as members of an Association which stands half-way between the ordinary mathematical teacher and the professional mathematician in the narrower sense, will probably agree with me that I am wiser to avoid topics of what is usually called a "pedagogical" character. I am sorry to be compelled to use the unpleasant word "pedagogical," and I am sure that you will believe me when I say that I do not use it in any contemptuous sense, and that I am enough of a pedagogue myself to realise the very genuine interest of many "pedagogical" questions. But I do not regard it as the business of a professional mathematician to concern himself primarily with such questions, and, even if I did, I should have very little to say about them. It has always seemed to me that in all subjects, and most of all in mathematics, questions concerning methods of teaching, whether this should come before that, and how the details of a particular chapter are best presented, however interesting they may be, are of secondary importance; and that in mathematics at all events there is one thing only of primary importance, that a teacher should make an honest attempt to understand the subject he teaches as well as he can, and should expound the truth to his pupils to the limits of their patience and capacity. In a word, I do not think it matters greatly what you teach, so long as you are really certain what it is; and I feel that you might reasonably be impatient with me, whether you agreed with me or not, if I occupied your attention for an hour and had nothing more to say to you than that. It is obviously better that I should take some definite chapter of mathematical doctrine, a chapter which is at any rate of the most obvious and direct educational interest, and expound it to you as clearly as I can.

It is, however, quite likely that some of you, and particularly any genuine geometer who may be present, will criticise my choice of a subject in a manner which I might find a good deal more difficult to meet. You might object that it would be reasonable enough for me to try to expound the differential calculus, or the theory of numbers, to you, because the view that I might find something of interest to say to you about such subjects is not *prima facie* absurd; but that geometry is, after all, the business of geometers, and that I know, and you know, and I know that you know, that I am not one; and that it is useless for me to try to tell you what geometry is, because I simply do not know. And here I am afraid that we are confronted with a regrettable but quite definite cleavage of opinion. I do not claim to know any geometry, but I do claim to understand quite clearly what geometry is.

I think that this claim is in reality not quite so impertinent as it may seem. The question "What is geometry?" is not, in the ordinary sense of the phrase, a geometrical question, and I certainly do not think it absurd to suppose that a logician, or even an analyst, may be better qualified to answer it than a geometer. There have been very bad geometers who could have answered it quite well, and very great geometers, such as Apollonius, Poncelet, or Darboux, who would probably have answered it extremely badly. It is a comfort, at any rate, to reflect that my answer can hardly be worse than theirs would in all probability have been.

I propose, then, to cast doubts of this sort aside, and to proceed to answer my question to the best of my ability. There are two things, I think, which become quite clear the moment we reflect about the question seriously. In the first place, there is not one geometry, but an infinite number of geometries, and the answer must to some extent be different for each of them. In the second place, the elementary geometry of schools and universities is not this or that geometry, but a most disorderly and heterogeneous collection of fragments from a dozen geometries or more. These are, or should be, platitudes, and I have no doubt that they are to some extent familiar to all of you; but it is a small minority of teachers of geometry that has envisaged such platitudes clearly and sharply, and it is probably desirable that I should expand them a little.

I begin with the second. It is obvious, first, that a great part of what is taught in schools and universities under the title of geometry is not geometry, or at any rate mathematical geometry, at all, but physics or perhaps philosophy. It is an attempt to set up some kind of ordered explanation of what has been humorously called the real world, the world of physics and sensation, of sight and hearing, heat and cold, earthquakes and eclipses; and earthquakes and eclipses are plainly not constituents of the world of mathematics.

It is dangerous to repeat truisms in public, and the particular truism which I have just stated to you is one which I have often expressed before, and which has sometimes been received in a manner very different from that which I had anticipated. But I am not speaking now to an audience of rude and simple physicists, or of philosophers dazed by centuries of Aristotelian tradition, but to one of mathematicians familiar with common mathematical ideas. I find it difficult to believe that any mathematician of the twentieth century is quite so unsophisticated as to suppose that geometry is primarily concerned with the phenomena of spatial perception, or the physical facts of the world of common sense. It is, however, perhaps unwise to take too much for granted, and I will therefore try to drive home my point by a simple illustration.

Imagine that I am giving an ordinary mathematical lecture at Oxford, let us suppose on elementary differential geometry, and that I write out the proof of a theorem on the blackboard. John Stuart Mill would have maintained that the theorem was at the best approximately true, and that the closeness of the approximation depended on the quality of the chalk; and, though Mill was a man for whom I feel in many ways a very genuine admiration, I can hardly believe that there is anybody quite so innocent as that to-day. I want, however, to push my illustration a stage further. Let us imagine now that a very violent dynamo, or an extremely heavy gravitating body, is suddenly introduced into the room. Einstein and Eddington tell us, and I have no doubt that they are right, that the whole geometrical fabric of the room is changed, and every detail of the pattern to which it conforms is distorted. Does common sense really tell us that my theorem is no longer true, or that the strength or weakness of the arguments by which I have established it has been in the very slightest degree affected? Yet that is the glaring and intolerable paradox to which anyone is committed who supports the old-fashioned view that geometry is "the science of space."

The simple view, then—the view which I will call for shortness the view of common sense, though there is uncommonly little common sense about it—the view that geometry is the science which tells us the facts about the space of physics and sensation, is one which will not stand a moment's critical examination; and this, of course, was plain enough before Einstein, though it is Einstein who, by enabling us to exhibit its paradoxes in so crude a form, has finally completed the demonstration. The philosophers, of course, have tried to restate the view of common sense in a more sophisticated form. Geometry, they have explained to us, tells us, not exactly the facts of physical or perceptual space, but certain general laws to which all spatial perception

must conform. Philosophers have been singularly unhappy in their excursions into mathematics, and this is no exception. It is, as usual, an attempt to restrict the liberty of mathematicians, by proving that it is impossible for them to think except in some particular way; and the history of mathematics shows conclusively that mathematicians will never accept the tyranny of any philosopher. The moment a philosopher has demonstrated the impossibility of any mode of thought, some rebellious mathematician will employ it with unconquerable energy and conspicuous success. No sooner was the apodeictic certainty of Euclid firmly established, than the non-Euclidean geometries were constructed; no sooner were the inherent contradictions of the infinite finally exposed, than Cantor erected a coherent theory. I do not think, then, that we need trouble ourselves with the views of the philosophers concerning geometry. They are, indeed, of much less interest than those of the man in the street, which do possess some interest, since there are valid reasons for supposing that others may share them.

It will be more profitable to leave the philosophers alone, and to consider what the mathematicians themselves have to say. We shall then have reasonable hope of making some substantial progress, since mathematicians, or those of them who are at all interested in the logic of mathematics, hold fairly definite views, and views which are in tolerable agreement, concerning this question of the relation of geometry to the external world. The views of the mathematicians are also much more modest than those which the philosophers have tried to impose upon them.

A geometry, like any other mathematical theory, is essentially a map or scheme. It is a *picture*, and a picture, naturally, of *something*; and as to what that something is opinions do and well may differ widely. Some will say that it is a picture of something in our minds, or evolved from them or constructed by them, while others, like myself, will be more disposed to say that it is a picture of some independent reality outside them; and personally I do not think it matters very much which type of view you may prefer to adopt. What is much more important and much clearer is this, that there is one thing at any rate of which a geometry is *not* a picture, and that that is the so-called real world. About this, I think that almost all modern mathematicians would agree.

This is only common mathematical orthodoxy, but it is an orthodoxy which outsiders very frequently misunderstand or misrepresent. I need hardly say that it does not mean that mathematicians regard the world of physical reality as uninteresting or unimportant. That would be on a par with the view that mathematicians are peculiarly absent-minded, always lose at bridge, and are habitually unfortunate in their investments. Still less does it mean that they regard as uninteresting or unimportant the contribution which mathematics can make to the study of the real world. The Ordnance Survey suggests to me that Waterloo Station, and Piccadilly Circus, and Hyde Park Corner lie roughly in a straight line. That is a geometrical statement about reality, and it enables me to catch my train at Paddington. Einstein is more daring, and issues his orders to the stars, and the stars halt in their courses to obey him. Einstein, and the Ordnance Survey, and even I, can all of us, armed with our mathematics, put forward suggestions concerning the structure of physical reality, and our suggestions will continually prove to be not merely interesting, but of the most direct and practical importance. We can point to this or that mathematical model, Euclidean or Lobatschewskian or Einsteinian geometry, and suggest that perhaps the structure of the universe resembles it, or can be correlated with it in one way or another; that that is a possibility at any rate which the physicists may find it worth their while to consider. We can offer these suggestions, but, when we have offered them, our function as mathematicians is discharged. We cannot, do not profess to, and do not wish to *prove* anything whatsoever. There is not, and cannot be, any question of a mathematician proving any-

thing about the physical world; there is one way only in which we can possibly discern its structure, that is to say the laboratory method, the method of direct observation of the facts.

I will venture here on an illustration which I have used before. If one of you were to tell me that there are three dimensions in this room, but five in Southampton Row, I should not believe him. I would not even suggest that we should adjourn our discussion and go outside to see. The assertion would, of course, be one of an exceedingly complicated character, and a very painstaking analysis might prove necessary before we were quite certain what it meant. However, I could attach a definite meaning to it. I should understand it to imply that, owing to particularities in the geography of London which had up to the present escaped my attention, the common three-dimensional model, sufficient for our purposes in here, becomes inadequate when we pass out into the street. And, however sceptical I might feel about such a theory, I should certainly not be so foolish as to advance mathematical arguments against it, for the all-sufficient reason that I am quite certain that there are none. I should be sceptical, not as a geometer but as a citizen of London, not because I am a mathematician, but in spite of it; and, indeed, I am sure that, if you appealed from me to the nearest policeman, you would find him not less but far more obstinately sceptical than me.

I must pass on, however, to what is really the proper subject-matter of my address. Geometries, I will ask you to agree provisionally, are *models*, and models of something which, whatever it may be in the last analysis, we may allow for our present purposes to be described as mathematical reality. The question which we have now to consider is that of the nature of these models, and the characteristics which distinguish one from another; and there is one great class of geometries for which the answer is immediate and easy, namely, that of the *analytical* geometries.

An analytical geometry, whether of one, two, three, four, or n dimensions, whether real or complex, projective or metrical, Euclidean or non-Euclidean, and it may, of course, be any of these, is a branch of analysis concerned with the properties of certain sets or classes of sets of numbers. I will take the simplest example, the two-dimensional Cartesian geometry which resembles very closely, though it is by no means the same as, the elementary "analytical geometry" taught in schools. I will call it, as I usually call it in lectures, *Common Cartesian Geometry*.

In Common Cartesian Geometry, a *point* is, by definition, a pair of real numbers (x, y), which we call its *coordinates*. A *line* is, again by definition, a certain class of points, viz. those which satisfy a linear relation $ax+by+c=0$, where a, b, c are real numbers and a and b are not both zero. The relation itself is called the *equation* of the line. If the coordinates of a point satisfy the equation of a line, the line is said to *pass through* the point, and the point to *lie on* the line. And that is the end of Common Cartesian Geometry, in so far as it is projective, that is to say in so far as it does not use the so-called metrical notions of distance and angle, and in so far as it is concerned only with equations of the first degree. What remains is just algebraical deduction from the definitions.

Common Cartesian Geometry, as I have defined it, is a very simple and not a very interesting subject. It gains a great deal in interest, as you will readily imagine, when "metrical" concepts are introduced. We define the *distance* of two points (x_1, y_1) and (x_2, y_2) by the usual formula

$$d=\sqrt{\{(x_1-x_2)^2+(y_1-y_2)^2\}},$$

and the *angle* between two lines by another common formula, which I need not repeat. We have still, however, only to explore the algebraical consequences of our definitions, and no new point of principle arises, so that I can illustrate what I want to say quite adequately from the projective and linear system. This system, trivial as it is, has certain features to which I wish to call your attention as characteristic of analytical geometries in general.

The first feature is this, that a point in Common Cartesian Geometry is *a definite thing*. This is so in all analytical geometries. Thus in any system of two-dimensional and homogeneous analytical geometry a point is a class of triads (x, y, z), those triads being classified together whose coordinates are proportional, and in the geometry of Einstein a point is a set of four numbers (x, y, z, t). This is a very obvious observation, but it is of fundamental importance, since it marks the most essential difference between analytical geometries and "pure" geometries, in which, as we shall see, a point is not a definite entity at all.

The next point which I ask you to observe is the absence of *axioms*. There are no axioms in any analytical geometry. An analytical geometry consists entirely of *definitions* and *theorems*; and this is only natural, since the object of axioms is, as we shall see, merely to limit our subject-matter, and in an analytical geometry our subject-matter is known.

It is most important to realise clearly that, in different geometrical systems, propositions verbally identical may occupy entirely different positions. What is an axiom in one system may be a definition in another, a true theorem in a third, and a false theorem in a fourth. You are accustomed, for example, to *proving* that the equation of a straight line is of the first degree, and I am not suggesting that the "proof" to which you are accustomed is meaningless, trivial, or false. You profess to be proving a theorem, and you are, in fact, genuinely proving *something*, though it might take us some time to ascertain exactly what it is. There is one thing, however, that is quite plain, and that is that the something which you are proving is not a theorem of analytical geometry, for your supposed theorem is, as a proposition of analytical geometry, not a theorem at all but the definition of a straight line.

Let us take another simple illustration, the "parallel postulate" of Euclid. *If L is a line, and P is a point which does not lie on L, then there is one and only one line through P which has no point in common with L.* This, in school geometry, is sometimes called an "axiom" and sometimes, I suppose, an "experimental fact." It cannot be either of these in analytical geometry, where there are neither axioms nor experimental facts, and it is obviously not a definition. It is, in fact, a theorem, which in Common Cartesian Geometry is true, though in other systems it may be false; and it is a theorem which any schoolboy can prove. It is the algebraical theorem that, given an equation $ax+by+c=0$, and a pair of numbers, x_0, y_0, which do not satisfy this equation, then it is possible to find numbers A, B, C, such that

(i) $$Ax_0+By_0+C=0$$

and (ii) the equations

$$ax+by+c=0, \quad Ax+By+C=0$$

are inconsistent with one another; and that the ratios $A : B : C$ are determined uniquely by these conditions.

These are the characteristics of Common Cartesian Geometry which it is most essential for us to observe at the moment. There are others which I should like to say something about if I had time. There is no infinite and no imaginary in this geometry; there are imaginaries, naturally, only in complex systems, and infinites in homogeneous systems. Further, the principle of duality is untrue. All these topics call for comment; and I should have liked particularly to say something on the subject of the geometrical infinite, since the tragical misunderstandings which have beset many writers of text-books of analytical geometry, and which have generated such appalling confusion in the minds of university students, are misunderstandings for which writers like myself of text-books on analysis have been largely though innocently responsible. The geometrical infinite, however, is a subject which would demand at least a lecture to itself. Apart from this, there is nothing in analytical geometry which presents any logical difficulty whatever, and I may pass to the slightly more delicate topic of pure geometry.

The nature of a system of pure geometry, such as the ordinary projective system, is most easily elucidated, I think, by contrast with analytical systems. The contrasts, which I have made by implication already, are sharp and striking, and when once they have been clearly observed the road to the understanding of the subject is open. I observed, first, that the points and lines of analytical geometry were *definite objects*, such as the pair of numbers (2, 3). Secondly, I observed that there were no *axioms* in an analytical geometry, which consists of definitions and theorems only; and that it is the definitions which differentiate one system of analytical geometry from another. The business of an analytical geometer is, in short, to investigate the properties of *particular systems of things*. The standpoint of a pure geometer is entirely different. He is not, except for incidental and subsidiary purposes, concerned with particular things at all. His function is always to consider *all things which possess certain properties*, and otherwise to be strictly indifferent to what they are. His "points" and "lines" are neither spatial objects, nor sets of numbers, nor this nor that system of entities, but *any* system of entities which are subject to a certain set of logical relations. The particular system of relations which he studies is that which is expressed by the *axioms* of his geometry. It is the axioms only which really matter; it is they which discriminate systems, and the definitions play an altogether subsidiary part.

Suppose, for example, if I may take a frivolous illustration, that a pure geometer and an analytical geometer were to go together to the Zoo. The analytical geometer might be interested in tigers, in their colour, their stripes, and in the fact that they eat meat. A point, he would say, is by definition a tiger, and the central theorems of my geometry are that "points are yellow," that "points are striped," and above all that "points eat meat." The pure geometer would reply that he was quite indifferent to tigers, except in so far as they possessed the properties of being yellow and striped; that *anything* yellow and striped was a point to him; that "points are yellow" and "points are striped" were the *axioms* of his geometry, and that all he wanted to know was whether "points eat meat" is a logical deduction from them.

You will, in fact, find, if you consult any standard work on pure geometry, such as Hilbert's *Grundlagen* or Veblen and Young's *Projective Geometry*, that a pure geometer begins somewhat as follows. We consider a system S of objects $A, B, C, \ldots$. We call these objects *points*, and their aggregate *space*; the *plane*, I may say, if I confine myself for simplicity to geometries of two dimensions. From the complete system S which constitutes space we pick out certain partial aggregates $L, M, N, \ldots$, which we call *lines*. If a point A belongs to the particular partial aggregate L, we say that *A lies on L* and that *L passes through A*. These are the *definitions*, and you will observe the quite subsidiary part they play. They are, in fact, purely verbal, and common to all systems; and they do not indicate or imply any special property whatever of the objects which they are said to define, which are indeed often called the *indefinables* of the geometry. The function of the definitions, in fact, is merely to point to the indefinables.

The serious business of the geometry begins when the axioms are introduced. We suppose next that our points and lines are subject to certain logical relations. These suppositions are assumptions, and we call them axioms. To construct a geometry is to state a system of axioms and to deduce all possible consequences from them.

Let us take an actual example. I select the following system of axioms:

AXIOM 1. *There are just three different points.*

AXIOM 2. *No line contains more than two points.*

AXIOM 3. *There is a line through any two points.*

These axioms are consistent with one another, for it is easy to construct a system of objects which satisfy them. We might, for example, take the numbers 1, 2, 3 as our points and the pairs of numbers 23, 31, 12 as our

lines, in which case all our axioms are obviously satisfied. Further, the axioms are independent of one another. If the numbers 1, 2, 3 were still our points, but the pairs 2 3, 3 1 alone, and not the pair 1 2, were taken as lines, then the first and second axioms would be satisfied but not the third, and it naturally follows that Axiom 3 is incapable of deduction from the other two. You will have no difficulty in proving in a similar manner, if you care to do so, that each of the three axioms is logically independent of the others. I do not profess to have stated the axioms in the best form possible, but at any rate they are consistent and independent.

It is easy to deduce from our axioms:

THEOREM 1. *There are just three lines*;

and THEOREM 2. *There are just two lines through any point.*

The state of affairs in this geometry is, in short, that suggested by a figure consisting of three points on a blackboard and three lines joining them in pairs. With this, our geometry appears to be exhausted.

The geometry which I have constructed is not an interesting system, since it has no particular application and virtually no content. For our present purpose, however, that is an advantage, as it makes it possible for me to exhibit the system to you in its entirety. However little interest it may possess, it is a perfectly fair specimen of a pure geometry. All systems of pure geometry, projective geometry, metrical geometry Euclidean or non-Euclidean, are constructed in just this way. They are usually very much more complicated, for you must naturally be prepared to sacrifice simplicity to some extent if you wish to be interesting; but their differences from my trivial geometry are differences not at all of principle or of method, but merely of richness of content and variety of application.

I have now given to you the substance of the orthodox answer to the question which I started by asking. I might expand it indefinitely in detail, but I should add nothing essentially new. Geometry is a collection of logical systems. The number of systems is infinite, and any of you can invent as many new systems as he pleases; I have myself, with the aid of a few pupils, constructed seven or eight in the course of an hour. There are two kinds of systems, analytical geometries and pure geometries. An analytical geometry attaches the usual geometrical vocabulary to more or less complicated systems of numbers, and investigates their properties by means of the ordinary machinery of algebra and analysis. A pure geometry, on the other hand, considers all possible fields of certain logical relations, and explores their connections without reference to the nature of the objects among which they hold.

I said when I started that I did not propose to offer any very definite suggestions about the teaching of mathematics; but I should like to conclude with a few words about some of the practical problems with which members of this Association are primarily concerned. It should be obvious to you by now, I think, that school geometry is, as I stated early in my address, not a well-defined subject, a rational exposition of a particular geometrical system, but a collection of miscellaneous scraps, a selection of airs from different pieces, strung together in the manner which experience shows to be the most enlivening. It would be very easy for me to illustrate my thesis by examining a few passages from current text-books of geometry. What is taught as projective geometry, for example, is not projective geometry, and makes very little pretence of being so, since it is based quite frankly on ratios of lengths and other obviously metrical concepts. Indeed, so far as I know, no English book on projective geometry proper exists, except Mathews' *Projective Geometry*, Dr. Whitehead's tract, and parts of Prof. Baker's treatise. On the other hand, a great deal of what is taught as analytical geometry is not analytical geometry, but an attempt to apply the methods of analytical geometry in other fields, partly to some rough kind of physical geometry supposed to be given intuitively, partly to some system of hybrid pure geometry of which

some previous knowledge is assumed. But I must not enter into detail, since detail would mean criticism, and criticism of particular books and particular passages, which I have no time for, and am in any case anxious to avoid.

It is not my object now to offer criticisms of the present methods of geometrical teaching. There are a good many very obvious criticisms suggested by the doctrines which I have tried to explain to you, but I recognise that most of these criticisms would be to a very great extent unfair. It is obvious that the teaching of geometry must be based on what is at best a very illogical compromise, and I am prepared to believe that the compromise evolved by experience, and applied by people who know a good deal more about the practical necessity of compromise than I do, is in substance as reasonable a compromise as the difficulties of the problem permit. My object, so far, has been one not of criticism but of explanation.

I do propose, however, to conclude with one word of criticism, directed only to those of you whose pupils are comparatively able and comparatively mature. There is no doubt that the standard of teaching of analysis has improved out of all knowledge during the last twenty years. The elements of the calculus, even the elements of what foreign mathematicians call algebraical analysis, are taught in a manner with which I personally have comparatively little fault to find. The stupid old superstition that falsehood is always easy and attractive, the truth inevitably repulsive and dull, is almost dead, and it is no longer supposed that ignorance of analysis is in itself a proof either of superior intelligence, or high moral character, or profound geometrical or physical intuition. The teaching of higher geometry does not seem to me to have advanced in the same degree.

I think that it is time that teachers of geometry became a little more ambitious. Geometry in its highest developments may be, for all I know, a more difficult subject than analysis; it is not for me as an analyst to deny it. But what may be true enough of the theory of deformation of surfaces, or of algebraical curves in space, is not even plausible of the elements of higher geometry. Those stages of the subject are surely very much easier than the corresponding stages of analysis. There is something hard and prickly about the basic difficulties of analysis, definite stages on the road where definite types of mind seem to come to an inevitable halt. The difficulties of geometry seem to me a little softer and vaguer; knowledge and general intelligence will carry a student appreciably further on the way. And, if this is so, it seems to me regrettable that students are not given the opportunity, while still at school, of learning a good deal more about the real subject-matter out of which modern geometrical systems are built. It is probably easier, and certainly vastly more instructive, than a great deal of what they are actually taught. Anyone who can investigate properties of six or eight points on a conic is capable of understanding what projective geometry is. Anyone who has the faintest hope of a scholarship at Oxford or Cambridge could learn the nature of an axiom, and how a system of axioms may be shown to be consistent with, or independent of, one another. And anyone who can be taught to project two arbitrary points into the circular points at infinity could learn, what he certainly does not learn at present, to attach some sort of definite meaning to the process he performs. Small as my own knowledge of geometry is, and slight as are my qualifications for teaching it to anybody, I have not yet encountered the student who finds difficulty with such ideas when once they are put before him clearly. I am well aware of the very great services which the Association has rendered in the improvement of geometrical teaching. I think that it might well now concentrate its efforts on a general endeavour to widen the horizon of knowledge, recognising, as regards niceties of logic, sequence, and exposition, that the elementary geometry of schools is a fundamentally and inevitably illogical subject, about whose details agreement can never be reached.

G. H. HARDY.

NOTE ON THE DISCUSSION ON TANGENCY AND LIMITS, AND ON THE GEOMETRY REPORT IN GENERAL.

Dr. W. F. SHEPPARD criticised the *Report* for connecting the ideas of a line touching a circle and a quadratic equation having equal roots. He thought that there was no real analogy, as the first was an undeniable fact, while the second was only a convention or a phrase invented by mathematicians to conceal the facts and gloss over an exceptional case.

Mr. KENWORTHY BROWN believed that the older methods of teaching geometry were preferable to those recommended in the *Report*. He believed that boys of 10 could with advantage commit to memory Euclid's definitions and the earlier propositions, and that Euclid's first proposition really appealed to them more than the somewhat formless course now in favour. He outlined a course for beginners, following Euclid much more closely than is now usual.

Prof. M. J. M. HILL discussed seven methods of deriving the properties of parallels from the postulate of similarity. All these methods required also Pasch's axiom (that if a straight line cuts one side of a triangle internally and does not pass through a vertex, it will also cut one of the other sides internally), but five of the methods, although due to distinguished mathematicians, were faulty in that they unconsciously made use of other assumptions or axioms in addition to those explicitly stated. He believed that the methods due respectively to Prof. Nunn and to himself avoided this defect.

Mr. BOON drew attention to the necessity of limiting the length of a great circle to make the enunciation of propositions in spherical geometry valid, and to a similar necessity in the corresponding system of Non-Euclidean Geometry.

Mr. WRIGHT rebutted the charge that the *Report* had not been considered and approved by the General Teaching Committee.

Prof. G. H. HARDY summed up the discussion. He regarded the *Report* as a very careful and reliable piece of work, of much higher standard than usual. The objections of Dr. Sheppard seemed without foundation. Mr. Kenworthy Brown's enthusiasm for learning definitions and propositions by heart was not shared by him. It should be realised that Euclid's definition of a straight line was not scientific, but merely a loose way of giving a rough idea of what was being discussed. Euclid's earlier propositions were not really serious mathematics at all. It was in his treatment of the later propositions that Euclid was so admirable.

He thought he agreed with all that Prof. Hill had said. Of course, it was fairly obvious that the whole of Euclidean Geometry could be deduced from the propositions common to Euclidean and Non-Euclidean Geometry with the addition of any one proposition that belonged exclusively to Euclidean Geometry. In fact, Lewis Carroll, in his *New Theory of Parallels*, had worked this out, taking as his proposition: the area of a square inscribed in a circle is greater than that of one of the four areas intercepted between the square and the circle.

From a practical point of view it was rather doubtful whether those elaborate investigations were worth while. The simplest axiom on which the properties of parallels could be based was: "Through a given point one and only one line can be drawn to be in the same plane as a given line (not passing through the given point) and yet not to meet it, however far produced."

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292. At Stourbridge Common was held a famous fair, which may be the prototype of Bunyan's *Vanity Fair*. Here Sir Isaac Newton bought for 3*l*. his three prisms, which at that time could have been manufactured only in France or Italy.

THE NEGLECT OF ARITHMETIC IN SCHOOLS.

By Professor J. E. A. Steggall, M.A.

For many years it has been my custom to take the junior students, that is to say the lowest class, in Mathematics for one hour a week: and a part of the session has been given to what may be called the simpler treatment of the principles of Arithmetic. No attempt has been made to touch the more difficult parts of this subject: no effort has been made to base, for example, the idea of fractions upon a purely abstract foundation.

Usually the simple facts of numeration, notation, groups of units, and so forth, are briefly considered, and some of the ordinary processes of calculation are dealt with in a little detail. Mere commercial sums are not discussed at all: an occasional illustration, of as practical a kind as possible, is made use of in its proper environment. About twelve lectures complete the course.

When it is considered that the students come from good secondary schools, it is surprising how very little they seem to know about the elementary notions on which any sound knowledge of Arithmetic must be based. Every allowance being made for young folk, who are expected to attend to a teacher who probably throws light upon the subject they are supposed to know in a direction different from that to which they have been accustomed, the result of my long experience is somewhat startling.

Of course, my experience is almost entirely drawn from the work of a Scottish University, and the knowledge of Arithmetic acquired in Scottish secondary schools; but in a not very remote past I have seen a good deal of the work of English schools, particularly when I was asked by the O. and C. Board to examine the thirty or so high schools for girls in Arithmetic. Here the lines to be followed by the questions were pretty clearly defined, and within these limits the work was of excellent quality throughout all the six papers of various degrees in difficulty that were set.

Although it is with the attainment of the highest pupils, those in a word who may be supposed fit to enter a university, that I am mainly concerned, it is probably desirable that some reference should be made to the most elementary work.

Nothing is more essential, I hold, in the very beginning than constant concrete illustration. This appears nowadays to be generally recognised—in fact, both books and teachers sometimes carry these illustrations so far that the wood cannot be seen for the trees; and it seems likely that in the case of children, young and old alike, the connection between the illustration and the fact is by no means always clear.

Still this is much better than the opposite fault which used to prevail: in many very large schools of a secondary character the younger children were asked to bring, as home work, such appalling sums in multiplication as the product of 937,640,052,318 by 7, by 29, by 3409; and any suggestion that such arrays of figures could not convey any meaning to children of eight or nine was met by the answer (1) that unless they proceeded say from an 8-figure multiplicand one term to a 10-figure multiplicand the next, the parents would consider no progress was being made; or (2) that this kind of thing cultivated accuracy.

On one occasion, in inspecting the work of a large class, I found that they had proceeded as far as compound multiplication and division of money: an example was given me—something like this—"Divide £319,285,317 16s. 11d. by 4923." It was obviously absurd to expect that in a few minutes this could be done correctly by the class: and to examine and weigh the nature of any errors made would be an impossibility. I therefore asked two questions: (1) A boy is given five pounds to spend on a week's holiday (pre-war); his board came to 8s. 6d. a day; he required 10s. for railway travelling; he

spent 2s. 6d. on each of three days in rowing on the sea; and 7s. on sundry small things. How much had he left after seven days? (2) A father gave £50 one Christmas to be equally divided between his 12 children. How much did each get?

Scarcely an answer was right, and the wildest errors were made, both of reasoning and of mere calculation.

The teacher was rather annoyed, but like a good fellow he bore no malice, and next year told me that on trying the sort of thing that was suggested he was extremely pleased with the result.

Another case was this: the class had reached addition of five rows of figures, five in each row. They were asked to add 305, 12764, and 2168, words being used. They first wrote down 3005, 12000700604, 2000100608. The actual figures were then written down, in a row. Their arrangement by the children in columns was all wrong, and finally the young woman in charge said with some asperity, "I beg your pardon, but they are accustomed to add five rows of figures with five figures in each row, and not these fanciful numbers." She evidently forgot that in actual life any numbers that require consideration are rarely presented in this very symmetrical fashion.

On the other hand, there were many teachers, chiefly women, who succeeded in imparting to very young children admirably clear and exact ideas as to notation and numeration: they rarely handled numbers of more than four digits.

But while at the outset concrete illustration is of the first importance, there is, or was, a good deal of time wasted in the miscellaneous problems that arise from the complex nature of our weights and measures. I venture to think that only such "tables" should be taught at any stage as are within the reach of practical experiment—and then they should be reduced to their simplest elements. Just as the knowledge of one inflected language, which in my young days was Latin, enables any student to grasp the general ideas underlying many others, so a clear apprehension of the nature and use of the various units employed in one specific table of measures will clear the way for a due comprehension of other systems. For example, feet, yards, chains, miles, although perhaps illogically connected, all have their particular uses, uses which, with reference to familiar cases, and even with reference to poetical and other literary allusions, are capable of quite interesting treatment. "Full fathom five thy father lies"; "Half a league, half a league, half a league onward"; the biblical use of "cubit" and "span" are cases in point.

It is needless to labour these simple considerations further, especially as I have in my mind not deficiency in elementary work, but deficiencies in older pupils. The late Dr. Casey, in the preface to his charming *Sequel*, of which the general format is quite unworthy of the work itself, speaks pathetically of the compulsion under which he is compelled to go back and explain to advanced pupils quite elementary details. This is exactly what in my experience is constantly necessary with the lower classes in a university. I am afraid, but I dare not dogmatise, because my opportunities of learning what is the real course of instruction in secondary schools are very limited, I am afraid that the tendency of recent "leaving examinations" is to assume that Arithmetic is so well taught that it is needless to examine upon this subject. It is quite true that the ordinary rules are fairly well known, and that pupils can quite readily work any ordinary sum by these rules; but if the least deviation is made there seems a great want of elasticity of mind, judging from the answers usually given.

To me there is no doubt that, as a part of general education, almost every student fit to enter a university can acquire a reasonable grasp of principles: it is equally clear that although at an early age a sound beginning has been usually made, at the later stages the work takes the form of preparation for a written examination; and I am bold enough to say that a written, as compared with an oral, examination tests least effectively the most important

part of mathematical study in its earliest development; and for this reason, I think, mainly: that to describe lucidly in writing much of the clearest and best thinking that a boy or girl can perform is utterly beyond the literary skill of much older people.

In my experience, then, this constant overshadowing immanence of the written examination leads to the neglect of some most important work, which cannot pay because of this want of literary skill: in turn, examiners, generously anxious to give students a chance, eschew the very kind of question that might best suggest a line of serious study.

The numerous interesting properties of integers, prime numbers, factors, how to determine them and the like, are little understood, and apparently carelessly studied. The beautiful theorem that if the multiples of any number, say 24, are successively divided by any other number prime to it, say 35, every possible remainder appears in a certain sequence without repetition, and that, therefore, there is one remainder unity; or, in other words, there are uniquely two numbers p, q less than 35 and 24 respectively, such that $24p - 35q = 1$, is well within the capacity of any ordinary boy who proceeds to the university: yet there are numbers of boys and girls in discussing De Moivre's theorem who can use precisely the reasoning required here, but have no idea of its simple application to common Arithmetic.

Much is made of decimals nowadays, but I doubt whether pupils generally realise that a decimal is a vulgar fraction written with a different convention; and I find that any ideas about circulating decimals are of the foggiest character.

I have no wish to depreciate the present work in schools in comparison with the past, but I am afraid I must maintain my position, which is that such matters of principle as I have touched upon are of vital importance in elementary mathematical education. To surrender this position is, to me, almost tantamount to saying that mathematics has no justifiable claim to a place in the university or higher school course of study except on the narrowest utilitarian and practical grounds.

When I was young, I am afraid it was fifty-five years ago, Mr. Herman Michael Kisch, a senior school-fellow, showed very great interest in my boyish studies, and lent me his copy of De Morgan's *Arithmetic*. I did not understand it all, studying it alone, but I well remember its fascination: the chapters on circulating decimals appeared most charming, and I think that every boy and girl who remains at school till eighteen or nineteen should know something of this beautiful part of Arithmetic. It is the easiest thing in the world, for example, to show that all proper fractions whose denominators are a given prime (excepting, of course, 2 and 5) must have the same number of places in their period, and that this number must be the denominator diminished by one or a sub-multiple of this number. The actual relations between the periods are most interesting; and to a fairly good mathematical mind the investigation of the more general case when the denominator is not prime is far from difficult. All this work is closely related to such elementary parts of the theory of numbers as are often included in text-books of Algebra. But in books on Arithmetic little attention is usually given to this work, although I have a recollection of a charming little volume by Mr. Candler who dealt with this subject in some detail.

The next point that I have selected for consideration is the process for extracting the square root. If there is any operation in arithmetical work of which the value lies almost wholly in its reason, this is the one. Nevertheless, in spite of the formal correctness of nearly all the explanations given in the books, I rarely find even an able student who can state, much less explain, the nature of the operation. Need I say that by this I mean (1) the discovery of squares, differing by one unit of a denomination proceeding steadily downward, between which the given square lies; (2) a method by which the next trial square is guessed or inferred, for the process is inverse; (3) the economic

calculation of each successive square as derived from its predecessor? I am sure I need not say that I do not suggest any cut and dried precision in the language of these statements. The books usually give the explanations without a very clear statement of the processes: and the pupils learn by heart the practical rule. But rarely, or never, do they grasp its practical use; least of all when one wishes to double the number of significant figures, already and accurately found, by the use of contracted division.

The reason for dwelling on this point is fairly clear: in much of the higher work in mathematics the remainder theorem, and its use in expanding numerically an integral algebraic function of $(x+a)$ in powers of x, is of constant application; and a little study of Horner's method for solving numerical equations gives valuable practice. No simpler case exists than that of the extraction of the square root, or, better still, the solution of a quadratic equation. Once the rationale of the method is grasped all numerical solutions of equations are equally reduced to rule; I do not, of course, advocate any very lengthened detailed work.

The last of the illustrations I give is the constant failure to make numerical calculations on any reasonable plan. For example, a very good kind of exercise, which incidentally suggests the idea of a convergent series, is to be found either in binomial formulae for roots, *e.g.*

$$2^{\frac{1}{3}} = \tfrac{5}{4}\,(1+3/125)^{\frac{1}{3}},$$

$$\sqrt{50} = 7\sqrt{(1+1/49)},$$

and so on; or in simple logarithmic series, such as

$$1/3 + 1/(3\,.\,3^3) + 1/(5\,.\,3^5) + \ldots.$$

I have set such questions as this:

"Evaluate the three series

$$1/5 + 1/(3\,.\,5^3) + 1/(5\,.\,5^5) + \ldots,$$

$$1/7 + 1/(3\,.\,7^3) + 1/(5\,.\,7^5) + \ldots,$$

$$1/3 + 1/(3\,.\,3^3) + 1/(5\,.\,3^5) + \ldots,$$

to six decimal places, and prove that, to this accuracy, the last is the sum of the other two."

I find more often than not that each denominator is worked out as an integer, and from the fraction the decimal is obtained—I need not say usually with several common errors, and without sufficient accuracy, thus:

$$1/(5\,.\,3^5) = 1/1215 = \cdot 0008, \text{ etc.}$$

I cannot, in conclusion, evade a depressing uncertainty as to the necessity or the usefulness of this short essay, which has touched in a very slight manner some of the many difficulties that surround the acquisition of elementary but sound mathematical ideas; and I am certainly conscious that many present have wider experience in dealing with school boys and girls than I could ever claim. The only defences I can erect for my temerity this morning are, first of all, the general principle that if one is asked by responsible persons to give an address on an occasion like this, it is more gracious to acquiesce than to make excuses; and next, I am a profound believer that the proper, and therefore the exact study, even of so humble a subject as elementary Arithmetic is a necessary, in fact the necessary, first step towards the culture of Mathematics; and, although it is not given to everyone to be a mathematician any more than to be an artist, a musician, or a poet, yet just as every normal educated man or woman is rightly expected to have some fairly correct notions on art, on poetry, and on music, so some reasonable, rational and exact knowledge of numbers and their properties is to be regarded, independent of all commercial and technical applications, as an essential element of the culture that all normal educated persons should have acquired.

J. E. A. Steggall.

THE REFORM OF UNIVERSITY MATHEMATICS.

By H. B. Heywood, D.Sc.

Those who have worked for the reform of mathematical education in this country, and the Mathematical Association in particular, have up to the present time concerned themselves mainly with reform in the school. A fairly complete survey of this field has been carried out. As a result the character of school mathematics has been transformed, the syllabuses, if not always the question papers, of most examining bodies have been regenerated, and even in the most academic haunts there has been a feeling of uneasiness. It would be unduly optimistic to suppose that the recommendations of the reformers have been universally adopted, or that there is not still a great deal to accomplish, but the progress achieved suggests that the time has come when the universities may be taken in hand.

There is another consideration which indicates university mathematics as the next objective for the reformer. The teachers in schools are university trained. An enlightened school course has been put within their reach to adopt if they will. But if they are to take full advantage of this course, their training also must be enlightened. The new task is therefore in a sense the complement of the task already partly accomplished.

The reform of university mathematics has not up to the present, I believe, been systematically studied by any corporate body in this country, but there have been several moves in this direction, and much that has been written upon school mathematics has its natural corollary in the university. Last May a leading article * appeared in *Nature*, which has a more close relation to our subject.

The writer drew attention to the unsatisfactory position of mathematical physics in this country, and urged that a course of experimental physics should be an integral part of the training of mathematical students. Professor Piaggio took up the thread of this article in a paper † published in the October number of the *Mathematical Gazette*, and discussed the mathematical physics of the lecture room in some detail. His estimate of the science, however, differed strikingly from that of the former writer, for whom, as for Perry, mathematics was a tool for use in dealing with concrete problems, or, more poetically, "the handmaid of natural philosophy." "Pure mathematics," says Professor Piaggio, "has asserted its claims to liberty and equality, but is inclined to forget the advantages of fraternity."

A very interesting and important innovation has been the establishment at the Universities of Glasgow and Manchester, and at the Imperial College, London, of laboratories furnished with the instruments used in numerical and graphical computation, which has thereby been raised to the status of an independent branch of mathematics. Professor E. T. Whittaker has named it the calculus of observations.

The remarks which I propose to offer are suggestions thrown out in the hope that some of them may be worthy of discussion, or may call forth other suggestions more worthy than themselves. I am concerned primarily with the normal university courses, such as are terminated by degree examinations.

The Need for Reform. British mathematics is in need of reform. There is no doubt that we are at the present moment a long way behind most civilised foreign nations. In the two articles to which I have referred it was admitted that in mathematical physics we have in recent years nothing to compare with the achievements abroad or with the achievements of our own people in past generations. Nor do I altogether agree with Professor Piaggio when he suggests that in pure mathematics we are drawing abreast of our colleagues

* *Nature*, 10th May, 1924, "Mathematical Physics in University and School."

† *Mathematical Gazette*, October 1920. Same title as (1).

abroad as a body. Integral equations and divergent series, subjects which he says are now being taught, were current coin in foreign universities twenty years ago, and so were the theory of aggregates, Lebesque integrals and integral functions. At that time we were painfully learning our first steps in the domain of the complex variable. To-day it is true we are at length beginning to learn the function of the real variable, but in continental and American universities courses are being given and investigations pursued on new subjects, such as functionals, of the most profound interest, of which we do not yet know the names. In geometry we have a rather better showing, for this country seems to have a special liking for geometry, but even here there are several great modern developments in which we have had no hand. I do not accept foreign achievements as a criterion of what we ought to do. We ought to do the best we can, and history tells us that when we try we can do very well indeed.

From another angle we are painfully aware that many of the followers of allied sciences, such as physics and engineering, covertly or openly laugh at us as hopelessly academic, and have come to the conclusion that at best mathematics is only a tool, though in our hands a rather blunt one.

There can be no question, then, that reform is urgently needed.

The University Teacher. One might be inclined to say, "Let the universities be; they may not be all they should be, but university teaching can flourish only if it is free, and you will only hamper it if you say it ought to be done this way or that way." I do not think we need say this. It is true that freedom is most essential to university teaching, but part of my argument will be that it is the lack of freedom that has brought about the present unsatisfactory condition, and part of my gospel will be more freedom for the university teacher.

All who have experience of university teaching know that it suffers far more restrictions than school teaching. The school teacher has a longer period at his disposal; he has less ground to cover; he is, if he so chooses, less dominated by examinations, and if he is enlightened there is much that he may do independently. He may, and often does, carry out the recommendations of this Association's reports. The university teacher has a bare three years, dominated by inexorable examinations in which to cover a large amount of work, whose scope and character are rigidly fixed by syllabus and by custom. If we recommend reforms, nay, if we had powers to impose them, he would welcome our benign tyranny after the tyranny under which he now labours. With post-graduate and other free teaching, such as has of recent years happily developed in all universities, we are not concerned. Here the teacher has scope; he is allowed to be alive; his inspiration has play, and it is his motive power.

The University Course. Let us consider what is the university course and how it has evolved. It divides itself up into a number of discrete parts, with such labels as algebra, conics, calculus, statics, dynamics. Each of these parts in turn is subdivided into theorems or "pieces of bookwork," of such a length that each takes about half an hour to write out. This last statement may seem a curious one, but I think that a perusal of any textbook or lecture course will convince anyone to whom it has not occurred that it is approximately true. If a theorem in its original form is too long, parts of it are left out or it is cut up into parts of the requisite length. With each theorem go a certain number of examples, usually specially constructed to illustrate the theorem. These examples, too, are of more or less standardised length. There are quite short ones that take only a few minutes to do, and there are longer ones that take about half an hour.

The Influence of Examinations. This curious subdivision of the subject, whether it is good or bad, is of course in the main the outcome of the examination system. It is mentioned as a visible proof of the way in which examina-

tions have moulded the subject-matter. Half an hour has for some reason or other become standardised as a convenient length for an examination question. The bookwork has adapted itself accordingly.

Examinations have not only moulded subject-matter, they have to a certain extent determined its nature. A branch of mathematics that does not lend itself to the setting of questions of standard length will be badly represented in the examination; it will, consequently, be neglected by teacher and student alike. Another branch which is more amenable will be overworked. It is a curious experience to have to set a question paper upon a new subject that has no textbook. At first one is puzzled; on reading through one's notes every section seems too long or too short, everything is bound up with something else. At length one makes out first one and then another piece of work, more or less complete in itself and of about the requisite length, which may be dissected out; and in the end, with a certain amount of mutilation, one produces the ten or twelve questions required, and one realises that, if the subject ever becomes a stock subject, these questions will represent the parts of it that will be taught.

A student who goes to a university is expected to get a degree. If he does not get a degree he will be called, and regarded as, a failure. If he does get a degree, that will be a starting-point for some sort of career. To get a degree he has to pass two examinations, and if he has only ordinary ability he finds that to pass those examinations will occupy all his energy and leave no margin for any other intellectual work. The teacher, on the other hand, is given a certain number of students, and is expected to prepare them for degrees, which means for examinations. If the students do not pass their examinations they will be dissatisfied, and the college authorities will also be dissatisfied, with the teacher. The teacher, therefore, to save his own skin becomes a coach. He is none the less a coach because he and his college may pretend he is not. The textbooks used may or may not be written as cram books. But it is certain that if a book is not suitable for examination purposes it will not be used.

Thus it is that whether they will or not, whether they know it or not, college authorities, teachers, students and textbooks all serve one inexorable master. What shall be taught and how it shall be taught is prescribed by him. And he is not a scientific master.

It appears, therefore, that if we wish to give to the teaching of mathematics in British Universities a real university character, the British examination system is the first obstacle that must be cleared. This is a preliminary work, which is necessary before any constructive reform can become effective.

The Reform of Examinations. How may we deal with the examination problem? The problem belongs to all university departments, although in our department it is most urgent. We might, of course, make examinations our instrument. Once gain control of them and we can force the colleges to teach anything we like. That method is being used, especially in school teaching, and used with effect. This Association has probably achieved more important results by bringing influence to bear upon examining bodies than it has by direct action upon teachers. While most examining bodies prefer to be followers rather than leaders, some have, in fact, exercised a powerful and very beneficial influence upon teaching in institutions that would otherwise have been very recalcitrant to reform. Parenthetically it may be remarked that these examining bodies have learnt a truth that is by no means known to all. They have learnt that the making of a new syllabus achieves little or nothing if the question papers that are set upon it are not also rejuvenated. Others, who have not learnt this truth, have hoped to find in their syllabus an incantation which, without further effort on their part, will perform the miracle. They are like Glendower, boasting:

"I can call spirits from the vasty deep,"

to whom we may counter with Hotspur:

> "Why, so can I, or so can any man;
> But will they come when you do call on them?"

But examining bodies at their best are oppressive, and we ought to deal with them as I understand King Henry II. dealt with his barons, while making use of them for his own purposes he at the same time step by step undermined their power. Can we do without examinations? Probably not. When we are all able to trust each other's judgment, perhaps we may. What we may try to do is to bring about some change that will subordinate the examination to the teaching.

It is not my purpose to enter into details, and I will here only make a few tentative suggestions of the way in which examinations might perhaps be mitigated. First we might discard the accepted principle that a student must be tested over the whole field of a syllabus. That he should know the whole syllabus is no doubt a good thing, especially if the syllabus is a sound one. But it might be a better thing to test the quality rather than the quantity of his knowledge. It might be a better thing that he should know part of the field very well, and a still better thing might be that he should be able to act upon this knowledge. In accord with this change of standpoint we might replace the papers of questions by papers of themes and of problems, only one of which was to be selected and developed at some length. Books of reference might or might not be allowed according to the nature of the themes, and the problems should not be ones that "come out." Other tests that might be used are the dissertation or essay upon a theme prescribed a year or so in advance, sent in with the teacher's guarantee, and subsequently defended orally; the lecture, in which a candidate is set a subject and provided with books, and is required, after a certain time given for preparation, to deal with the subject orally for ten minutes or so; a test in which by computation or drawing candidates have to produce numerical results in a workable form; in branches of mathematics which admit of it, a laboratory test.

It may be objected that few students could sustain oral tests of the character suggested; that nervousness and lack of power of self-expression are difficult to deal with, and difficult to assess and to allow for. There is much ground for this objection at present. But the main reason for nervousness in a healthy person is a lack of practice and of training. If oral tests were established in degree examinations, students would then be trained to speak. And how much better training it is to stand up before a class and expound one's ideas than to sit listening day after day to other people's lectures. The one is an active exercise of the mind, the other passive reception. But there is more than this; a large proportion of the students will become teachers, and all who continue to take an active interest in their work will subsequently have to explain and expound their ideas to others. In the oral training that I advocate lies a valuable preparation for their future work.

Another means that might be employed when a student comes from an institution with a reputation is a report sent in by his teachers. In many universities such a report is already taken into account in the granting of degrees. I do not think it unreasonable that a degree should be granted to a student on the recommendation of a small committee of his teachers without any further test, when those teachers are of sufficiently high standing. The report of the committee might be handed to the student with his diploma, and, in addition to securing him a degree, it would take the place of the usual testimonials.

Most or all of these methods of testing students are in successful operation in one place or another, and their more general adoption might have a very beneficial effect upon university teaching.

The Influence of Tradition. It will, however, be wise not to exaggerate the influence of the examination system. It has not so much made university

mathematics what it is as prevented it from becoming what it might be. Turning from form to content, we know that the most serious defect of the existing course and the basis of the charge which is preferred against it by the outside world is its "academic" character. A large part of it is found only in the lecture room and the textbook. The student will never meet with it again unless he too becomes a teacher, and then only when he is imparting it to another generation of students. If he becomes a mathematician, it will not assist him in his investigations. If he becomes an engineer, it will render him no service in his professional work, and he will forget it.

This academic character is due no doubt to many causes, but chief among them is habit or tradition. A teacher, unless he be an exceptional man, will teach what he has been taught. The examiner will use his predecessors' question papers as models. The writer of a textbook will copy other textbooks. Thus subjects will become strongly entrenched, and continue to be prescribed and taught when the trend of the science has long been in another direction.

The Perry Controversy. And here I may refer to what may be called the Perry controversy, after one of the first and greatest champions in the contest, the quarrel between those who say that mathematics is a tool for the use of physicists and engineers and those who consider that usefulness is unworthy of a good mathematician.

The division between the academic mathematicians and the practical mathematicians is not so acute as it formerly was. Each party has perceived some virtue in the other's contentions. But the question is far from being settled, and it will perhaps prove the most serious obstacle in any scheme of reform. By no means all the realm of mathematicians is involved in the controversy. There are very many whom it hardly interests. But its importance and its potency for progress lies in the fact that it is a movement from outside. And for this reason all who have mathematics at heart ought to be interested in it. The view which I think we should take is that both sides are wrong in their doctrines, but to a certain point right in their practice.

It is not very easy to convince the Philistine who asks, "If your mathematics have no practical application, what then is the use of them?" Such attacks as this have put us on our defensive, and led us to make pathetic apologies which are not always quite sincere. We point out that many pieces of pure mathematics have had unexpected applications, and we cite instances: that the geometry of conic sections was developed long before it was suspected that planets moved in ellipses; that Levi-Civita did not know that his work would be the foundation-stone of relativity. Or we may urge, as did Emile Picard in an address which he once delivered before the Rice Institute, that even were other justification wanting, mathematics was undoubtedly of great philosophic and artistic interest. Note how we, falling in with the mood of the Philistine, try to justify our science in terms of something else.

But if the use of mathematics lies in physics, where lies the use of physics? In engineering, perhaps? Then where lies the use of engineering? Is it in earning dividends for shareholders? It is true that the report of the chairman of the directors generally makes very clear what he considers to be the function of the engineer. He has his own point of view. But there is assuredly no body of professional men more devoted to their science than engineers. It is a glorious thing to build a great turbo-generator. For the real enthusiast there the matter generally ends, and I am afraid that such considerations as making a fortune or improving the conditions of human life are for him often adventitious.

To improve the conditions of human life is the worthiest of all motives, which might well direct the whole effort of a scientific man. But as the basis for a discussion of what is or is not worth doing it is useless. To begin with, no definite meaning can be assigned to the phrase. According to the geo-

graphers the prime necessities of human life are food, shelter and clothing. According to Dean Inge material progress is not progress at all. The truth is that science *is* life, even as the winning and eating of food and the speculations of Dean Inge. Human life is a complex of many activities, many motives and aspirations, which it is the function of the teacher to foster and develop in the disciple. The teacher is upon difficult ground when he attempts to judge from the standpoint of his personal doctrines whether some activities are more worth developing than others. The mathematician when challenged may attempt more or less unsuccessfully to justify himself in terms that will be understood by people whose pursuits are different, but he himself does not require this justification.

The Unity of Science. The trouble arises from the way in which we try to split up human activity into compartments, and then think of the contents of one compartment as if they were themselves a whole. The whole is indivisible; the part cannot be considered alone. Our mistake has been in trying to draw a borderline between mathematics and physics, and, when we have found the two inextricably intertwined, in inventing a kind of mathematical physics which was to be, by definition, mathematics. There is no borderline between mathematics and physics; there is a borderland which pertains to both.

There is no need for mathematics to be an untidy appendage to physical theory; there is no need for physics to become fictitious when it furnishes elements for mathematical treatment. There are not two kinds of mathematical physics, in one of which mathematics is a tool while in the other physics is an illustration. There is only one kind, and it is at the same time essentially mathematical and essentially experimental. For us physicists and engineers *are* mathematicians, and ought to accept the full obligations of this honourable title; we, on our part, have similar obligations.

In this conception of the continuity and unity of science lies, I think, the solution of the Perry controversy. The controversy is not concerned with the whole of mathematics at all, but only the parts of it that overlap the allied sciences. The present system in which there is a department of mathematical physics or applied mathematics which never sees a galvanometer or a testing machine, departments of physics and engineering which treat mathematics with irony, and odd classes where nondescript stuff is taught as "mathematics for engineers," ought to be abolished. There is room here for only one subject. It is at once analytical and experimental; it belongs at once to the laboratory or the workshop and to the lecture room.

This, then, is my second suggestion: an investigation of the common ground of mathematics and the allied sciences, carried out with the unity and continuity of science as a guiding principle.

The Persistence of Mathematics. Let us now return to our own ground. The curse of mathematics from the teacher's point of view is that it is all *right.* Other sciences periodically cast their skins. Physics and chemistry are regenerated every fifty years or so; new observations call forth new hypotheses, in terms of which they may be classified and interpreted; the old theories are replaced by new theories based upon the new hypotheses, and they gradually disappear from the university courses. The science of medicine every three or four years comes forth with a brand new skin, and its cast skin is thenceforth of interest only to the historian whose trade it is to collect such things. In mathematics, broadly speaking, everything that has ever been accepted as true still is and always will be true, and we must carry along with us all this vast accumulation of the workers who have gone before.

But if all mathematics is true, and surely it is all interesting, it does not all possess vitality. Mathematics is a great branch of the tree of knowledge with many moribund branchlets. How shall we know where the sap runs strongest? Mathematics is a road from which moss-grown by-ways lead off in every direction. Very great is the temptation to ramble along these

pleasant by-ways and at the end come nowhere. Out of all this mass of learning what shall we teach? There is much that is elegant, much that is beautifully logical, much that we may think is highly educational. But these considerations do not give us the guidance that we need. The university course is short. We must lead our pupil along the highway. When the course is through, he may, if he is fortunate, follow the by-way that pleases him, be it moss-grown or clear.

We must teach with a purpose. We are leaders, and must know where we are leading. We must guide our pupils to the places where the great investigators are at work, where the science is alive and growing. We must teach them the things that enter into the life and work of men. They ask for bread; we must not give them stones, however pretty.

The Doctrine of Mental Training. It is maintained by some of those who love pedagogy that mathematics is an excellent training for the mind. No doubt it is. It is urged that the study of mathematical theorems will produce a probity in thinking such as cannot be acquired in other ways, that practice in the solution of problems will bestow the power of resolving the difficult situations in life. It is claimed that those who have undergone a mathematical course often become able administrators and effective men of action.

The truth of these contentions we need not discuss. But we must point out that to suggest that one kind of mathematical course is a better mental training than another is pure surmise. It is no longer argued, as it used to be, upon vague assumptions that such a subject as pure geometry is especially excellent in this respect. The discussion has passed into the realm of psychology. Now psychology has so far reached only the first stage of human learning. If it has proved anything, it has proved that we know nothing about the human mind, and that all the hypotheses concerning it made heretofore are without good ground. The great modern advances in pedagogy are empirical; they are not founded upon general hypotheses.

The Principle of Vitality. Hence my thesis is that in deciding what should be taught in a mathematical course we should be guided only by the principle of vitality, and not at all by any consideration of mental training.

This leads to my third and principal suggestion: a purposeful study of the whole domain of mathematics to harness together our ideas, to display the science as an organic structure, a survey of the field to show where active development is taking place, where the science is alive and growing. The survey must embrace not only the sciences of analysis and geometry, which are all our own, but also the experimental and applied sciences, regarded as branches of mathematics. For our purpose, what is being taught in the lecture room must be ignored. We want to know what is being done outside the lecture room: what the savants are doing, what the outside world is doing. We must work in the spirit, though not on the scale, of the great *Oxford Dictionary*. We must discover what are, not what ought to be, the activities of mathematical workers, just as the dictionary shows us what are, not what ought to be, the meanings of the words in the English language. And when we know what is the mathematical work of the world, we can retrace our steps and say what should be the training which shall prepare a student to play his part in this work.

It would, I think, be a mistake to attempt to enunciate courses in the form of syllabuses. Not only would this be playing into the hands of the examiners, but it would be contrary to the spirit of university teaching. Our survey is designed not to provide the teacher with ready-made courses, but with objectives and materials, so that he may construct his own courses in accordance with his aspirations and the ambitions of his students. The courses that are now thrust upon him have no true objectives at all. If a student asks, as he has a right to ask, "Why am I doing this?" there is really no other answer than, "Because it is prescribed, and you will be examined in it."

The teacher ought to be able to look upon his students as future analysts, geometers or engineers, and say, "Because you will need it in your life-work."

I have made three suggestions which may at first sound a little revolutionary, but I have not forgotten the fortunate fact that it is useless to propose anything revolutionary to Englishmen. Existing institutions always have merits, which are as deep-seated as their defects are patent. No artificial institution can ever survive unless it can somehow acquire these obscure and human merits, and our English way is to alter the defective institution a little bit at a time, so that it comes a little nearer to what we desire.

I make only a plea for enquiries which shall discover where, and especially how, reforms may be brought about, so that when university authorities in a mood of divine discontent feel that all is not quite well, they may look to these enquiries, and say: "Yes, here, and perhaps here, we may be able to make a little improvement."

Biographical Note.

Professor Perry throughout his life waged war against academic mathematics, or, as he called it, "the ordinary abominable system of mathematical study," which he considered extremely bad for engineering students. His conception of mathematics was a tool for use in the solution of concrete problems, but he idealised this conception, and there is no question that his influence was as good as it was powerful and effective. Perhaps the best exposition of his views is the address which he delivered as President of the Engineering Section in the British Association meeting of 1902. The late Mr. C. Godfrey, one of the greatest members of our own Association, devoted most of his work to the schools. His views, however, have a wider range, and a paper which he read at the International Congress of Mathematicians at Rome in 1908, in which he discussed the rival claims of utility and mental discipline, has bearing upon our subject (*Mathematical Gazette*, May 1908). Professor G. A. Gibson, in his inaugural address at Glasgow (*Nature*, 25th November, 1909), compared the deductive and inductive aspects of mathematics. He remarked that, while mathematics usually became deductive in its definitive form, the road to discovery was usually inductive, in the philosophical sense. He recommended induction as suitable for teaching the elementary parts of the science. Most of the teaching in schools is now (in 1925) done upon inductive lines. Professor Percy Nunn, who writes mainly upon school subjects, takes what I may call the university standpoint, although I am not sure that he always bears it out to its full implications. In an article on "The Aim and Method of School Algebra" (*Mathematical Gazette*, December 1911), borrowing a dictum applied by Prof. Baldwin to physics, he says: "Your aim in the teaching of mathematics should be—to teach mathematics." He rebuts at once "the mental training" doctrine and the utilitarian doctrine. "Mathematics is to be taught because it is in itself a desirable possession. Mathematics is, like literature and art, a specific form of fine human achievement, one of the cardinal modes in which man has found himself and expressed his possibilities." He says of his course of algebra: "The professed aim is to make school mathematics a reproduction, as faithful as the difference of situations permits, of the mathematical activities of the great world."

We may also observe the modern tendency to break down the partitions by which human knowledge has hitherto been segregated into compartments. In the school nowadays we tend to teach not several independent subjects, labelled algebra, geometry, and so forth, but one subject labelled mathematics. There are now, also, general textbooks on mathematics for university students, notably one by Professor Hardy. And in this connection I may draw attention to the presidential address delivered in January 1924 by Professor Smithells before the Science Masters' Association, in which he discusses the influence of professional training in universities upon school teaching. He

considers that the division of natural knowledge into formal sciences is largely artificial, and instances the way in which any natural phenomenon is intertwined with several formal sciences.

NOTE ON THE SUBSEQUENT DISCUSSION.

Prof. E. W. HOBSON thought that Dr. Heywood had taken an unduly pessimistic view of the extent to which University teaching was dominated by examinations. At the University of Cambridge there had been a great improvement in the last twenty years in the direction of freedom of teaching. Dr. Heywood's view that England was better at geometry than analysis was rather surprising. England had never had a really great geometer.

A MEMBER (from University College, London) complained that although London University had recently greatly improved their syllabuses for honours courses in mathematics, yet the examination papers were of the same type as before, and this bound down the teacher to the old methods.

ANOTHER MEMBER thought that the present courses leading to final honours examinations at Oxford and Cambridge were valuable in that they prepared men for careers of various kinds, whereas the highly specialised course desired by Dr. Heywood would be useless except for the future specialist.

There was some suggestion that examinations could be made a little more elastic by giving alternative subjects in one of the papers, on one of which an essay could be given. Dr. Heywood seemed to think that a student should be allowed to work at any branch he chose, and should be examined on that branch: a personal examination, in fact, by his own teacher. Prof. A. Lodge thought that examination which, as now, tested general knowledge of mathematics up to a certain point was quite necessary, and that specialised research in the higher regions of mathematics should be post-graduate work.

Prof. H. T. H. PIAGGIO thought that the great range of subjects required in such examinations as the Mathematical Tripos caused overwork with a consequent loss of enthusiasm. It would be better that a young graduate should acquire a good grounding and a love for the subject that would lead him to continue his studies after he had left college, than that he should be strained to attain an unduly high standard at twenty-two years of age and never go any further.

Prof. G. H. HARDY agreed with Prof. Hobson that University teaching had improved greatly in the last twenty years. The teaching of analysis had improved immensely; formerly it was very bad indeed, but now it was very good. However, on the whole, he agreed with Dr. Heywood. The duty of a mathematical teacher at a university was to find out the truth and impart it to his pupils, and to devise courses with a definite objective so as to take part in the growth of the science. It was emphatically not his duty to arrange a course consisting of subjects which were only valuable for the mental training they imparted, or were supposed to impart. With regard to the very difficult honours examinations, like the Mathematical Tripos or Oxford Greats, he thought they were fundamentally vicious and should be abolished.

The system of devising elaborate examination tests for the purpose of drawing fine distinctions between one good student and another was a bad one. If a distinction was needed, it would be made by their work in the future. In Sweden there was an examination in elementary mathematics and another in advanced mathematics. The advanced one was severe, but those who chose to take it either passed or failed; there was no further classification.

Dr. HEYWOOD, as one who was not a Cambridge man, hesitated to follow Prof. Hardy in advocating the abolition of the Mathematical Tripos. It might happen that this abolition would have unforeseen disadvantages; and that it would be more desirable to endeavour to get rid of the evils to which their attention had been directed.

THE USE OF LIMITS IN ELEMENTARY GEOMETRY.

By C. V. Durell, M.A.

The pronouncement on the use of limits in the Report on *The Teaching of Geometry in Schools* is as follows: "There is no need to discourage the use of limits in the informal stages of geometry, but they are not suitable at the stage of formal demonstrations."

This clear-cut statement is the verdict which follows a reasoned examination of the claims made for the use of limits. The importance of this conclusion justifies a review of the enquiry. The Report maintains that, in the absence of a Newtonian or Weierstrassian treatment, the method of limits is logically indefensible, as it involves the use of a term of which no exact definition has or can be given at that stage. The force of this argument is weakened by the inconsistency of those who use it.

Take the standard geometrical proof of the theorem that "the portion of the tangent to a conic intercepted between the curve and the directrix subtends a right angle at the focus." On this result all future tangent properties are based.

Does any standard school text-book give, or any pupil learn, or any examiner require a proof of this theorem for the conic of greater validity than attach to the methods (A) and (B) [p. 44 of the Report], for the tangent properties of the circle, described there as *illustrations* but refused recognition as *proofs*? The ordinary proof of this property of the conic hinges on the theorem on the ratio in which the bisector of an angle of a triangle divides the opposite side. This theorem fails in the ordinary sense if the angle vanishes. Yet no one demands that the proof of this conics-property should be based on the Euclidean conception of a tangent. It has been said that pupils who tackle this theorem are so much advanced in their work that they can be trusted to understand the process employed. But it cannot be maintained that even at this stage they could express the process with that exact analytical precision which the pure mathematician demands.

The Committee may, therefore, reasonably be required to meet a charge of inconsistency, in that they exclude limit proofs from the formal stage in elementary geometry while admitting them as valid in formal geometrical conics. [The Report does not, of course, explicitly admit them in geometrical conics, it does not mention them; but the practice is so general that presumably no member of the Committee would disown them.]

Moreover, by allowing that limits may enter into the informal stage, the Committee seems to agree that pupils at that stage can form some idea of what a limit represents. The idea is, of course, incomplete throughout the formal stage (15-17), and remains incomplete throughout the school stage even of the specialist. But all the time he is gaining familiarity with the concept, although it is long before he can express it with a clear understanding in analytical terms.

Another argument urged in the Report deserves notice: on page 46 we read, "it is simply not true that tangents to conic sections are introduced invariably or even most frequently by a limiting-process." [The Report is referring to analytical geometry.] The Report then proceeds to state what analytical method is generally used. It is difficult to understand how this statement has been allowed to remain. It is doubtful, judging by current text-books, whether even 1 per cent. of those who learn analytical geometry *approach* the study of tangents by either of the methods given in the Report. This criticism is not irrelevant, for it is this statement which is used to dismiss the general consideration that the employment of limit methods in later work renders it advantageous to pupils to gain some familiarity with the idea at an early stage. In this connection the Report ignores another important

application, the graphical treatment of velocity and acceleration, which now enters into most non-specialist courses.

There is one argument against the use of limits which the Report does not mention; it is this:

In any examination full marks for a theorem on tangents will be obtained by a higher percentage of those who give Euclidean proofs than of those who give limit proofs, even with a sympathetic examiner. The reason is obvious: the Euclidean method of proof has a familiar form, and is, therefore, relatively easier to reproduce; the limit method is strange and needs more care in expression, and, therefore, more pupils stumble over it.

The following reasons support the adoption of limit proofs in the formal stage:

(i) The pupil is introduced to an aspect of geometry he has never seen before. The novelty of the idea stimulates his imagination.

(ii) The method emphasises the relation between chordal and tangent properties, and may be used to enable him to discover the latter for himself.

(iii) Whenever the idea of a limit first arises, its meaning will almost certainly be elucidated by geometrical analogies: it is, therefore, an appropriate element in a geometrical course.

(iv) The method prepares the way for measurement of slopes of curves and applications in kinematics: it is also intimately connected with the idea of incommensurable magnitudes, of which some mention will no doubt be made.

It is perhaps worth remark that no modern school text-book follows Euclid's method of treating the tangency of lines and circles in all its thoroughness, and probably many teachers would fail (if asked) to reproduce Euclid's chain of reasoning. It makes an attractive logical argument, but work of this nature is more suited to a Sixth Form syllabus than to the stage under consideration.

The treatment of the limit theorems will vary naturally with the ability and maturity of mind of the class.

In the initial stage, and for formal reproduction, the cinematographic treatment indicated in the Report, suitably expanded and expressed, should be sufficient. Proofs of this nature are given in many current text-books. If these proofs arouse discussion in class, or if a suitable opportunity occurs, it may be profitable to discuss the subject in more detail. The remainder of this article suggests one way of doing so. It is not intended for reproduction in an examination, but as a basis for an elementary discussion on the theory of limits.

I. *The idea of a limit.*

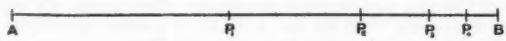

FIG. 1.

Draw a line AB, one decimetre long. Bisect AB at P_1; bisect P_1B at P_2; bisect P_2B at P_3; etc.

Then $AP_1=\frac{1}{2}$ dm.; $AP_2=(\frac{1}{2}+\frac{1}{4})$ dm.; $AP_3=(\frac{1}{2}+\frac{1}{4}+\frac{1}{8})$ dm.; etc.

We can repeat the bisection process as often as we like; the successive distances from A of the points "P" measured in dm. are

$$\tfrac{1}{2};\quad \tfrac{1}{2}+\tfrac{1}{4};\quad \tfrac{1}{2}+\tfrac{1}{4}+\tfrac{1}{8};\quad \tfrac{1}{2}+\tfrac{1}{4}+\tfrac{1}{8}+\tfrac{1}{16};\ \ldots.$$

However often the process is repeated, we never arrive at B.

$\therefore$ however many terms in the series $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, ... are taken and summed, the result is always less than 1.

But by repeating the process sufficiently often, we can obtain a point P as near B as we like, for $P_1B=\frac{1}{2}$, $P_2B=\frac{1}{4}$, $P_3B=\frac{1}{8}$, ... And by taking a

sufficient number of terms in the series $\frac{1}{2}, \frac{1}{4}, \frac{1}{8}, \ldots$, we can obtain a sum as near 1 as we like: and *further*, any greater number of terms will give a sum still nearer to 1.

We therefore say that the point P_n tends to B as its limiting position, or that the limit of P_n is B when n increases indefinitely. The limit is, however, never attained, because the point P_n never coincides with B however often the process is repeated.

We also say that the *limiting sum* of the series $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\ldots$ is 1, or that the sum of the series tends to 1 when the number of terms increases indefinitely. But the limit 1 is never attained, because, however many terms are taken, their sum is always less than 1.

II. *The tangent as a limiting chord.*

A is a given point on a circle. Any line AQ through A cuts the circle again at P.

Bisect arc AP at P_1; bisect arc AP_1 at P_2; bisect arc AP_2 at P_3; etc.

We can repeat this bisection process as often as we like. We thus obtain a succession of lines AP_1Q_1, AP_2Q_2, AP_3Q_3, ..., which cut off from the circle arcs of decreasing lengths. However often we repeat the process we cannot obtain a line cutting off an arc of zero length; but by repeating the process sufficiently often we can obtain a line which cuts off an arc as short as we please, and all *further* lines will cut off still shorter arcs.

The limiting position AT of this series of lines is called the *tangent* at A, which is therefore the limiting position of a line through A cutting off an arc whose length tends to zero.

If the process of repeated bisection is performed on the other side of the chord APQ, we shall obtain the limiting position AS, which is equally (by our definition) the tangent at A. It is therefore necessary to prove that AS and AT are in one straight line.

The fact that the limiting position of AT does not depend on the position of the first line APQ will be proved by showing that there is only one tangent at A, *i.e.* there is only one line through A which cuts off from the circle an arc of zero length.

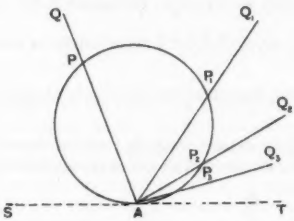

FIG. 2.

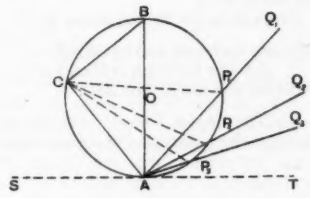

FIG. 3.

III. *The tangent-theorems.*

(i) The tangent at any point of a circle coincides with the line perpendicular to the radius through that point, at its extremity.

O is the centre of the circle; AOB is a diameter; AT is the tangent at A.

To prove $\angle BAT = 90°$.

Bisect arc AB at P_1; bisect arc AP_1 at P_2; bisect arc AP_2 at P_3; ....

Then the lines AP_1Q_1, AP_2Q_2, AP_3Q_3, ... form a series of lines whose limiting position is the tangent AT.

Method (1).

Take any point C on the circle on the other side of AB from P. Then the lines CP_1, CP_2, CP_3, ... form a series of lines whose limiting position is CA.

We therefore have two series of angles: (i) $B\hat{A}Q_1$, $B\hat{A}Q_2$, $B\hat{A}Q_3$, ..., and (ii) $B\hat{C}P_1$, $B\hat{C}P_2$, $B\hat{C}P_3$, ..., which tend to the exhaustion of the angles BAT, BCA respectively.

But $B\hat{A}Q_1 = B\hat{C}P_1$, $B\hat{A}Q_2 = B\hat{C}P_2$, $B\hat{A}Q_3 = B\hat{C}P_3$, etc.

$\therefore$ in the limit $B\hat{A}T = B\hat{C}A$.

But $B\hat{C}A = 90°$, angle in a semicircle; $\therefore$ $\angle BAT = 90°$. Q.E.D

Method (2).

Join OP_1, OP_2, OP_3, ...

$B\hat{A}P_1 = \frac{1}{2}B\hat{O}P_1 = \frac{1}{2}$ right angle, since arc BP_1 = arc P_1A.

$P_1\hat{A}P_2 = \frac{1}{2}B\hat{A}P_1 = \frac{1}{4}$ right angle, since arc $P_1P_2 = \frac{1}{2}$ arc BP_1.

$P_2\hat{A}P_3 = \frac{1}{2}P_1\hat{A}P_2 = \frac{1}{8}$ right angle; and so on.

$\therefore$ $B\hat{A}P_n = (\frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \ldots$ n terms) right angle.

By taking n sufficiently large we can make the sum of these terms approach 1 as near as we like.

But AT is the limiting position of AP_n when n increases indefinitely;

$\therefore$ $B\hat{A}T = 1$ right angle. Q.E.D.

Corollary. There is not more than one tangent at any given point on a circle; and every other line through that point cuts the circle at a second point.

The process of repeated bisection on the other side of AB leads to the limiting position AS as the tangent at A.

By the same argument, $S\hat{A}B = 1$ right angle;

$\therefore$ $S\hat{A}B + B\hat{A}T = 2$ right angles and SAT is a st. line.

Further, any other line AX through A must be either in the angle BAT or the angle BAS.

Suppose it lies in the angle BAS. Then, since $B\hat{A}X < 1$ right angle, a line AP_n can be found so that $B\hat{A}X < B\hat{A}P_n$;

$\therefore$ AX lies between AB and AP_n, and must therefore cut the circle at some point on the minor arc BP_n.

(ii) The angles which a tangent to a circle makes with any chord drawn through the point of contact are equal to the angles in the alternate segments.

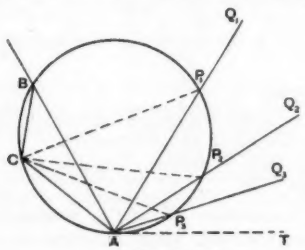

FIG. 4.

AB is a chord, AT is the tangent at A.

To prove $B\hat{A}T = A\hat{C}B$ in alt. segment.

Bisect arc AB at P_1; bisect arc AP_1 at P_2; bisect arc AP_2 at P_3, ...

Then the lines AP_1Q_1, AP_2Q_2, AP_3Q_3, ... form a series of lines whose limiting position is the tangent AT.

Also the lines CP_1, CP_2, CP_3, ... form a series of lines whose limiting position is CA.

We therefore have two series of angles: (i) $B\hat{A}Q_1$, $B\hat{A}Q_2$, $B\hat{A}Q_3$, ..., and (ii) $B\hat{C}P_1$, $B\hat{C}P_2$, $B\hat{C}P_3$, ..., which tend to the exhaustion of the angles BAT, BCA respectively.

But $B\hat{A}Q_1 = B\hat{C}P_1$, $B\hat{A}Q_2 = B\hat{C}P_2$, $B\hat{A}Q_3 = B\hat{C}P_3$, ...;

$\therefore$ in the limit $B\hat{A}T = B\hat{C}A$. Q.E.D.

C. V. DURELL.

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293. "Those concerned in the Publication, considering the great Use and Excellency of Mathematical Sciences, upon which, whatever of Certainty in others, purely Human, generally depends, thought they could do nothing of more universal Advantage, than to promote the Acquisition of a Knowledge so vastly beneficial, by all Methods within the Sphere of their Business. To this Purpose nothing appear'd more proper, than some entire System of Mathematicks, that might lead the Studious of such Knowledge, from the very first Principles, to the highest Pinnacle of Perfection, without being oblig'd to interrupt their Progress, by turning aside after other Books and Authors. . . . Those that subscribe shall have their Names printed before the same, as Encouragers of so useful a Work." Preface to Ozanam's *Recreations*. London, 1708.

294.

Sans doute vous serez célèbre
Par les grands calculs de l'algèbre,
Où votre esprit est absorbé.
J'oserais m'y livrer moi-même;
Mais hélas! $A + D - B$
N'est pas = à je vous aime.

—Voltaire to Madame du Châtelet.

295. In his youth his delight was in mechanical contrivances, and not having the means to buy mathematical instruments, he made two pairs of compasses for pencil and pen, hammered out of bits of sheet iron. His tutor was so pleased with them that he caused them to be laid on the table at the annual distribution of prizes of the Mechanics' Institute, and as the result Mr. Isaac Pitman publicly presented Holyoake with a proper case of mathematical instruments. After this Holyoake's name was placed on George Stephenson's list of young engineers, and of this he was very proud, though nothing came of it.—George Jacob Holyoake [*Sacrobosco* redivivus!].

296. Fiddle, Faddle, don't tell me of this or that, and everything in the world, but give me mathemacular demonstration, answer me directly.—(Lady Plyant, *log.*) Congreve's *Double Dealer*, ii. 1.

297. Jacques Lafitte, a banker, introduced the omnibus to Paris in 1819. The title is said to have been invented by Baudry, a retired officer, who eventually failed in his enterprises and committed suicide. The *carrosses à cinq sous* first appeared in Paris in 1662, and their origin is attributed to Blaise Pascal. He may have died of the shock.

THE TANGENT TO A CONIC.

By Prof. T. P. Nunn, D.Sc.

In every elementary treatise on the coordinate geometry of the conic sections one expects to find a treatment of two allied problems: (i) the deduction of the equation to the tangent at a given point of the curve, and (ii) the determination of the condition that a given line may be a tangent. The procedure generally followed is that of Salmon and C. Smith. For (i) we seek first the equation to a chord of the curve, and then enquire what form it will assume when the end-points of the chord coalesce; for (ii) we find the points where the given line cuts the curve, and then find the relation between the parameters of the line when the two points of intersection become identical. When the grounds for the procedure are explained, the justification usually offered is that a tangent is "the line joining two indefinitely near points on the curve"—a definition so lacking in precision that even Salmon's authority does not justify its use. The arguments based upon it are, in fact, open to the objections urged in the recent *Report on the Teaching of Geometry* against the premature use of the method of limits, and ought to be abandoned in favour of some method conducive rather than obstructive to clear thinking. The following method, based upon Euclid's definition of a tangent as a line which "meets the curve, but being produced does not cut it," has occurred to me (and probably to others) as satisfying this condition. I work it out first for the central conic, and apply it afterwards to the easier case of the parabola.

Central Conic.

Any curve corresponding to the equation

$$ax^2+by^2=1, \quad \text{.....(A)}$$

where a is assumed to be positive while b may be either positive or negative, is obviously symmetrical about the x-axis. Let P be any point on it, and P' the symmetrical point at the other end of the chord through P perpendicular to the x-axis. Then any point Q between P and P' is said to be inside the curve, and every point inside the curve is a point between the ends of some chord perpendicular to the x-axis. If a point is neither on nor inside the curve it is said to be outside the curve. A tangent to the curve at a point P thereon is a line through P whose other points are all outside the curve.

Let (x, y) be a point on the curve and (x, y') a point inside the curve. Then it follows from the definition that $y^2>y'^2$, and, from equation (A), that, if a and b are both positive,

$$ax^2+by'^2<1,$$

while if a is positive and b negative

$$ax^2+by'^2>1.$$

Conversely, if the relevant inequality holds good, the point (x, y') must be inside the curve. And since (A) is satisfied for a point on the curve, it follows further from the definition that if in the first case

$$ax^2+by'^2>1,$$

or in the second case

$$ax^2+by'^2<1,$$

the point (x, y') must be outside the curve. Conversely, since (the numbers being all real) $ax^2+by'^2$ must be less than or equal to or greater than unity, it follows that if (x, y') is outside the curve, the second pair of inequalities holds good, each for the relevant case.

Now let the equation of a line be

$$px+qy=1. \quad \text{.....(B)}$$

Then the lines joining the origin to the common points of the loci (A) and (B) are given by the equation

$$ax^2+by^2-(px+qy)^2=0, \quad \text{.....(C)}$$

and the condition that there shall be only one common point is that the left-hand side of (C) may be a perfect square; *i.e.*

$$aq^2+bp^2=ab \quad \text{or} \quad p^2/a+q^2/b=1. \quad \text{.....(D)}$$

In that case (C) reduces to

$$aq^2x^2/b-2pqxy+bp^2y^2/a=0,$$

whence

$$y=aqx/bp. \quad \text{.....(E)}$$

Substituting for y in equation (B), we obtain for the coordinates of the single common point

$$x=p/a, \quad y=q/b. \quad \text{.....(F)}$$

Next let $(x+h, y+k)$ be any point on (B) other than the point (x, y) or $(p/a, q/b)$, which it shares with the conic (A). Then, since

$$p(x+h)+q(y+k)=1,$$

and also

$$px+qy=1,$$

it follows that

$$ph+qk=0. \quad \text{.....(G)}$$

Also

$$\begin{aligned} a(x+h)^2+b(y+k)^2 &= a(p/a+h)^2+b(q/b+k)^2 \\ &= (p^2/a+q^2/b)+2(ph+qk)+ah^2+bk^2 \\ &= 1+(ah^2+bk^2) \quad \text{(by (D) and (G)).} \end{aligned}$$

Now if, as in the cases of the circle and the ellipse, a and b are both positive, (ah^2+bk^2) must also be positive, and we may conclude that the point $(a+h, y+k)$ is outside the curve. But if the conic is a hyperbola b will be negative, and it is not obvious that (ah^2+bk^2) has always the same sign. We can, however, show that in this case it is necessarily negative.

Since

$$ph+qk=0,$$

we have

$$k=-ph/q,$$

and

$$\begin{aligned} ah^2+bk^2 &= h^2(a+bp^2/q^2) \\ &= (aq^2+bp^2)h^2/q^2 \\ &= abh^2/q^2 \quad \text{(by (D)),} \end{aligned}$$

which is necessarily negative since a and b are of opposite signs.

We conclude that in all cases the line $px+qy=1$ is a tangent to the conic (A) at the point $(p/a, q/b)$; for it has this point in common with the conic, and all its other points have been shown to be outside the curve.

Let the coordinates of a point on the conic be x', y', and let $px+qy=1$ be the tangent at that point. Then, since by (F)

$$p=ax', \quad q=by',$$

the equation of the tangent can be written in the form

$$ax'x+by'y=1.$$

Again, let the equation of a tangent be

$$y=mx+c.$$

Then

$$m=-p/q, \quad c=1/q;$$

i.e.

$$q=1/c, \quad p=-m/c.$$

Substituting in (D), we obtain

$$c=\pm\sqrt{m^2/a+1/b}.$$

That is, the line

$$y=mx\pm\sqrt{m^2/a+1/b}$$

is a tangent for all values of m.

It may be well to add that the cases of the circle, the ellipse and the hyperbola, taken together here for the sake of brevity, would in actual teaching be considered separately.

Parabola.

In applying the same method to the parabola it must first be seen that the inequality

$$y^2 - 4ax > 0$$

implies that (x, y) is a point outside the curve. We then proceed to find in the usual way that the line $y = mx + a/m$ has only one point in common with the parabola, namely, the point $(a/m^2, 2a/m)$. Let $(x+h, y+k)$ be any other point on the line; then since

$$y + k = m(x+h) + a/m$$

and

$$y = mx + a/m,$$

it follows that

$$k = mh.$$

Now

$$(y+k)^2 - 4a(x+h) = (2a/m + k)^2 - 4a(a/m^2 + h)$$
$$= k^2.$$

From which we conclude that all points of the line except $(a/m^2, 2a/m)$ are outside the curves; that is, that the line is a tangent.

Finally, let the coordinates of the point of contact be written x', y', so that $x' = a/m^2$, $y' = 2a/m$; and let the equation of the tangent be multiplied throughout by $2a/m$. Then we have

$$2ay/m = 2ax + 2a^2/m^2;$$

that is

$$yy' = 2a(x + x').$$

T. P. NUNN.

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298. Fontenelle, in his panegyric on Sir Isaac Newton, closes a long enunciation of that great philosopher's virtues and attainments with an observation that "he was not distinguished from other men by any singularity, either natural or affected."

It is an eminent instance of Newton's superiority to the rest of mankind that he was able to separate knowledge from those weaknesses by which knowledge is generally disgraced; that he was able to excel in science and wisdom, without purchasing them by the neglect of little things; and that he stood alone merely because he had left the rest of mankind behind him, not because he deviated from the beaten track.—Dr. Johnson, *The Adventurer*, No. 131.

299. (Review of I. Todhunter's *Plane Coordinate Geometry*.) Mr. Todhunter shows a most commendable concern for the improvement of his readers, points out the matters requiring their careful attention, and expresses a kindly sympathy with their difficulties, which he does his best to alleviate. . . . —*Athenaeum*, Aug. 25, 1855.

300. Euclid says nothing about the extreme points of a line; he says "a straight line is one which lies ἐξ ἴσου with respect to the points on it," that is, to *all* the points on it.—*v.* G. B. Mathews, *Nature*, vol. 69, pp. 409, 459, 535.

301. A cock-and-bull story has been going the round of the ladies' papers to the effect that a little girl of ten has rewritten Euclid, and that favourable reviews of her work have appeared, attributing it to a clever man. We have the best authority for saying that no such book and no such reviews have appeared.—*Athenaeum*, April 2, 1892.

MATHEMATICAL NOTES.

750. [C. 2.] I shall be grateful for an evaluation of the integral

$$\int \frac{x\,dx}{(1-e^{-ka/x})a^3/x}, \quad a \text{ and } k \text{ constants.}$$

It has recently occurred in some work in Physics. W. PARNELL-SMITH.

751. [X. 10.] *Infinite* —— ?

Several members of the Association suggest that we should place on record in the *Gazette* the "curious fact" mentioned below in a quotation from a letter which appeared in *The Times*, Jan. 29, 1925. We have brushed aside the suggestion of one of them that this is a case for an offer of "a house for life" for the discoverer of the "simple equation." There are institutions in which free accommodation is already provided by a benignant State for the accommodation of sufferers of the calibre the discovery requires.

"Aristotle's spherical universe was finally developed by the Stoics, for whom it was a finite body, containing finite bodies. Dr. Jeans seems to return to this conception, though I do not know whether he will insist on the sphericity. But this finite body, containing all other finite bodies, is itself contained—by what? Beyond, said the Stoics, was nothing. But this "nothing" then becomes of some importance, because it contains the universe of things. It is clearly infinite, in the precise sense that we can set no bounds to it; and so it is no thing. This infinite, which is truly no thing, is what I was taught to call space, regarding the universe of things, and all its points and distances, as in space.

"I have found some corroboration of this way of thinking in the curious fact that the sum of an infinite series of numbers, $1+2+3+4+$ etc., can be shown by a simple equation to be zero; which seems, however, to be only another way of saying that the infinite is not anything finite. . . ."

752. [J. 2. c.] *Minoru.*

In this game two horses, Minoru and St. Amant, start four steps behind the winning-line, Game Chick 5 steps, Miss M'Giggle 7, and Gou-Gou 8 steps behind the winning-line.

At each turn one horse moves forward one step, and each of the five horses has an equal chance of being chosen for this honour.

Find each horse's chance of reaching the winning-line first.

W. HOPE-JONES.

753. [V. 1. a. μ.] The following useful fact does not seem to be sufficiently known to students, and should, I think, be brought into prominence.

If $\frac{1}{n}$th part of a mass has its centre of gravity moved through a distance x, the centre of gravity of the whole mass is thereby shifted a parallel distance $=\frac{1}{n}x$.

In fact, if the centre of gravity of the part traverses any path, the centre of gravity of the whole traverses a similar and similarly situated path, $\frac{1}{n}$th of the linear dimensions, the centre of similitude being the centre of gravity of the stationary remainder of the mass.

(As an instance of the utility of the theorem, consider the shift of the centre of gravity of a plane lamina when part of it is folded round any line.)

Another thing that worries me is the confusion between M and W, seen in so many scripts, and not always absent from books.

The equation of ratios $\frac{P}{W}=\frac{f}{g}$ is much simpler for beginners than

$$P=Mf \quad \text{and} \quad W=mg.$$

If both gravitational and absolute systems are taught, it should be insisted on that the letters M and W should be kept to represent Mass and Weight respectively, and that they should never be confused; also, that no numerical question should have both M and W in it.

To go from absolute to gravitational units we replace everywhere M by $\frac{W}{g}$, and, conversely, replace W by Mg. Also, never write 1 lb. weight $=g$ poundals as some books do. It is spoiling g, which is an acceleration and not a number.

Now that the *Gazette* has taken up dimensions so wholeheartedly, it might insist on accuracy of language in these respects.

We may write $Mv=Pt$ as a *physical* equation, and it will also hold numerically if absolute units are used, but if we want grav. units, the numerical equation is $\frac{Wv}{g}=Pt$; and, similarly, $\frac{1}{2}Mv^2=Ps$ may be written, and will be true numerically in absolute units, but in grav. units $\frac{1}{2}Mv^2$ must be replaced by $\frac{Wv^2}{2g}$, beloved of engineers, and more comprehensible to pupils, since $\frac{v^2}{2g}=h=$the height to which the body could rise if it were deflected upwards by frictionless constraints without shock.

[The fundamental mechanical engineers' units are not M, L, T, but W, L, T.] A. L.

754. [C. 1.] *Note on Note* 730.

The Senate House Problems and Riders, 1878 (Tuesday, Jan. 15th, 9 a.m. to 12 noon), p. 98, question 6, gives a result which contains my identity as a special case. It reads:

$$\left(\frac{d}{dx}\right)^m x^{m-r}\left(\frac{d}{dx}\right)^r x^{-m}\left(\frac{d}{dx}\right)^{n-r}\phi(x)=x^r\left(\frac{d}{dx}\right)^{m+n}\phi(x).$$

Write r for n, n for m in the above and it reduces, when $\phi(x)=x^n(x-1)^{t+r}$, to the identity I submitted.

Jacobi used a similar identity (put $n=t+r$ in the above) in his work on Legendre functions (he uses factors $1+x$, $1-x$ of course) while Schendel used a special case of it as a basis for a paper on a generalised Legendre function.

The transformation of operators is undoubtedly the more powerful method of approach. It must contain a fund of results which, given in concrete form as examples of operations, would interest an average class as "some pretty results."

I take this opportunity of thanking Mr. W. H. E. Bentley for supplying the reference I needed. W. L. FERRAR.

The University, Edinburgh.

755. [I. 2.] I. 47, *and numerical exercises.*

A very general method of finding right-angled triangles with rational sides is to write for the said sides:

$$n, \quad \frac{n^2}{p}\sim\frac{p}{4}, \quad \frac{n^2}{p}+\frac{p}{4};$$

n and p being any numbers we please.

This especially is useful when we want two right-angled triangles with a common side.

In particular, the well-known elementary method of finding the area of a triangle, in which we calculate the perpendicular from the vertex on the base by considering the segments into which the base is divided, is wholly free from irrationals if we take for sides :

$$\frac{n^2}{p}+\frac{p}{4},\quad \frac{n^2}{q}+\frac{q}{4},\quad \frac{n^2}{p}\sim\frac{p}{4}+\frac{n^2}{q}\sim\frac{q}{4};$$

when the area is $$\frac{n}{2}\left[\frac{n^2}{p}\sim\frac{p}{4}+\frac{n^2}{q}\sim\frac{q}{4}\right].$$

These formulae may interest those who, like myself, feel that it is positively immoral to set elementary numerical exercises on I. 47, which involve a great deal of tedious and irrelevant arithmetic ; and who find the concocting of problems soluble in rational numbers only rather troublesome.

B. C. Atkin.

Hereford College, Oxford.

756. [K¹. 5.] *The Relation between the Area of a Triangle and that of the Equilateral Triangle into which it can be orthogonally projected.*

Let the triangle ABC be orthogonally projected into the equilateral triangle DEC lying in a plane through C. Take as axes the lines, Cx, Cy, Cz, which are the internal and external bisectors of the angle DCE, and a perpendicular to the plane DCE, respectively. Then the co-ordinates of A, B, C are respectively : $$(k,\ k\sqrt{3},\ m),\quad (-k,\ k\sqrt{3},\ n),\quad (0,\ 0,\ 0)\ ;$$

where $2k$ is the side of the triangle DEC, $m=AD$, $n=BE$.

With the usual notation for the triangle ABC, we have

$$a^2=4k^2+n^2,\quad b^2=4k^2+m^2,\quad c^2=4k^2+(m-n)^2,$$

whence $$a^2+b^2+c^2=12k^2+2(m^2+n^2-mn).$$

If S and T be the areas of the triangles DEC, ABC, respectively, then (since $T=\frac{1}{2}ab\,.\,\sin C$),

$$4T^2=3k^2(m-n)^2+k^2(m+n)^2+12k^4,$$

that is $$T^2=k^2\,.\,(m^2+n^2-mn+3k^2)$$

and $$S^2=3k^4,$$

whence $$T^2+S^2=k^2(m^2+n^2-mn+6k^2)$$
$$=S(a^2+b^2+c^2)/2\sqrt{3}.$$

Therefore S is the smaller root of the quadratic

$$S^2-S(a^2+b^2+c^2)/2\sqrt{3}+T^2=0.$$

S. L. Green.

East London College, E 1.

757. [V. 1. a. μ.] *A Simple Dynamical Experiment.*

A coin is placed on a horizontal board, which is then moved in its plane with uniform velocity u. The coin lags behind (or, appears to move in the opposite direction) until friction has given it the velocity u. If the board be now suddenly stopped, the coin will go forward to its original position.

Thus, with ft.-sec. units, if $u=8$, $g=32$, coefft. of friction $=\frac{1}{2}$, the coin will move with acceleration 16 and will acquire the speed of the board in $\frac{1}{2}$ sec. It will have moved through $\frac{1}{2}(16)(\frac{1}{2})^2$ feet, *i.e.* 2 feet. Hence the coin will be $(4-2)$ ft. behind its original position. The board stopping, the coin goes forward with velocity 8 against a retardation 16, and comes to rest in $\frac{1}{2}$ sec., having moved over 2 ft., *i.e.* returned to its original position on the board.

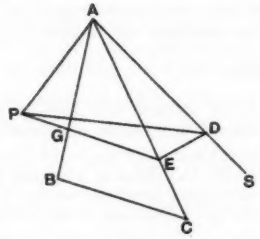
It may be of interest to add that the lag is always half the distance moved through by the board, no matter what numbers are chosen ; because the average speed of the coin is half that of the board. R. W. Genese.

758. [V. 1. a. μ.] *A Class Experiment in Hydrostatics.*

Water is poured into a long U-tube. The ends are closed by the thumbs. The tube is then raised into a vertical position, and the thumbs removed. The water is stationary. A slight inclination is given to the tube, so that the surface of the water in one branch becomes higher than that in the other: and then the water rises in the former and flows out through the latter.

R. W. GENESE.

759. [K^1. 1.] *To Bisect a Triangle by a Straight Line passing through a Given Point outside the Triangle.*



Let ABC be the triangle, P the given point. Join AP, make $C\hat{A}S = P\hat{A}B$, and on AS take rect. $AP \,.\, AD = \frac{1}{2}AB \,.\, AC$. Join PD, and on PD describe a segment of a circle containing an angle $= 180 - B\hat{A}D$. Let this segment cut AC in E. Join PE meeting AB in G. Then PGE is the line required.

Proof. $AGED$ is cyclic quad.

$$\therefore\ P\hat{G}A = A\hat{D}E;$$

$$\therefore\ \triangle s\ APG \text{ and } ADE \text{ are similar};$$

$$\therefore\ \frac{AP}{AG} = \frac{AE}{AD};$$

$$\therefore\ AP \,.\, AD = AG \,.\, AE = \tfrac{1}{2}AB \,.\, AC;$$

$$\therefore\ \triangle\ AGE = \tfrac{1}{2}\triangle\ ABC.$$

Hence GE is line required.

J. TRAVERS, B.A.

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760. [A. 1. a.] *Note on Note* 735 (*Gazette*, xii. p. 206).

Referring to Mr. Brooks' method of finding the cost of an annuity, it may be interesting to note that there is no need to use a Geometrical Progression at all.

Let C be the capital sum required to produce a perpetual annuity. The annual interest on C is $\frac{r}{100}C$; this is the annuity, and may be written $(\cdot 0r)C$.

If the annuity is for n years, the present value of the capital sum at the end of that period, viz. $C \div (1 \cdot 0r)^n$ must be deducted from C to find the cost of the annuity, *i.e.* its present value.

Finally, if p is the annuity, $C = \frac{p}{\cdot 0r}$;

$$\therefore\ \text{the cost} = \frac{p}{\cdot 0r}\left(1 - \frac{1}{(1 \cdot 0r)^n}\right).$$

In Mr. Brooks' example, $p=£10$, $r=4$, $n=5$;

$$\therefore \text{ cost} = £\frac{10}{\cdot 04}\left(1-\frac{1}{(1\cdot 04)^5}\right).$$

Note. $\frac{1}{\cdot 0r}$ is called the number of years purchase of a perpetual annuity: if $r=4$, the value is 25 years purchase, and so on.

So the method is to multiply p by the right number of years and by the factor $\left(1-\frac{1}{(1\cdot 0r)^n}\right)$.

[Another method of argument is that the cost equals the present value of the accumulated interest, *i.e.* $\frac{1}{(1\cdot 0r)^n}$ of $C\{(1\cdot 0r)^n-1\}$]. A. L.

761. [L 1.] *Commensurable Approximations.*

In dealing with this subject by means of simple continued fractions, Chrystal, Serret, and Weber all rely on a lemma that if a/b, c/d are positive rational numbers, such that $ad\sim bc=1$, then no fraction can lie between them unless its denominator is greater than both b and d. The proof which they give is simple enough, but a still more straightforward proof leads to a much more satisfying conclusion.

Suppose $a/b>p/q>c/d$; then $aq-bp>0$, $dp-cq>0$, and therefore

$$aq-bp\geqslant 1, \quad dp-cq\geqslant 1. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1)$$

Hence by simple multiplications and additions, eliminating q and p in turn,

$$(ad-bc)p\geqslant a+c, \quad (ad-bc)q\geqslant b+d,$$

and if $ad-bc=1$, then $\quad p\geqslant a+c, \quad q\geqslant b+d. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(2)$

Moreover, unless *both* the relations in (1) are equalities, *both* the relations in (2) must be inequalities.

If a/b, c/d are positive rational numbers such that $ad\sim bc=1$, then every rational number between them except $(a+c)/(b+d)$ has both a numerator greater than $a+c$, and a denominator greater than $b+d$.

The result, of course, is not new, but as a matter of curiosity I should be interested to learn of any text-book which gives it. E. H. NEVILLE.

762. {O. 3. b.} *The Law of Refraction for the Geometrical Lens System.*

In a paper recently communicated to the Physical Society, Mr. G. Temple, of Birkbeck College, showed how the characteristics of a symmetrical optical system could be deduced from the principles of homography and symmetry. In lecturing on optics it appeared to me that it would be useful to examine what modifications would have to be introduced into the properties of an actual system of lenses in order that the geometrical constructions involved in the theory of principal planes and principal foci should be geometrically accurate instead of approximate. The necessary assumptions are as follows.

(1) The surfaces at which refraction takes place are plane instead of spherical.

(2) The normal to a refracting surface is replaced by the radius drawn to the refracting plane from a fixed centre O (the erstwhile centre of curvature of the surface). The perpendicular from O on the plane is the axis.

(3) The incident and refracted rays and the corresponding radius all lie in one plane, and the law of refraction is expressible by the formula

$$\frac{\mu\sin\phi}{\cos\theta}=\frac{\mu'\sin\phi'}{\cos\theta'},$$

where ϕ and ϕ' are the inclinations of the rays to the radius, θ, θ' their inclinations to the axis.

If these assumptions are made the cross-ratio of the pencil formed by the incident and refractive rays, the radius and the refracting plane will always be equal to $\mu' : \mu$; rays proceeding from a source will after refraction accurately pass through a conjugate focus on the same radius as the source, subject to the formulæ

$$\frac{\mu}{v} - \frac{\mu}{u} = \frac{\mu' - \mu}{r} \quad \text{or} \quad \frac{\mu'}{p} - \frac{\mu}{q} = \frac{\mu' - \mu}{r},$$

where r is the length of the radius through them. Objects and their images will be strictly projective, and this last property will of course be true after any number of refractions, even in an unsymmetrical system in which the refracting planes are not coaxial. Any such system will be anastigmatic, rectilinear and possessing flatness of field and free from distortion (other than projective). A symmetrical system of coaxial refracting surfaces will give a perfect image of a plane figure perpendicular to its axis.

The advantage of starting with this assumption is that it shows clearly the origin of the errors of an actual lens system which it is the object of opticians to correct by a process of compensation, namely:

(1) The small distance separating the point of incidence on a spherical lens surface from the assumed point of incidence on the ideal refracting plane.

(2) The fact that in the actual law of refraction, unity takes the place of $\cos\theta$ and $\cos\theta'$ of the assumed law, the differences being of the second order in θ, θ'.

I should hardly imagine that this method of treatment is novel, but I have never seen it in print, and its publication may be of use by introducing clearness into methods of teaching the subject. Another point which the textbooks do not sufficiently emphasise is that the transformation produced by any system of coaxial lenses is merely compounded of a refraction through an equivalent lens and a displacement of translation through the space separating the two principal planes.

G. H. BRYAN.

763. [J. 2. c.] *Whist-Hands.*

I am indebted to Sir John Heaton for pointing out to me an omission in my list of the most frequent types of whist-hand (July Number, page 148).

A.	21·55 %	of hands are of the type	4, 4, 3, 2.
B.	15·52 %	,, ,, ,,	5, 3, 3, 2.
C.	10·58 %	,, ,, ,,	5, 4, 2, 2.
D.	10·54 %	,, ,, ,,	4, 3, 3, 3.

Between *B* and *C* I should have mentioned 12·93 % of the type 5, 4, 3, 1.

Eton College, Windsor. W. HOPE-JONES.

764. [K¹. 6. a.] *Note on Oblique Axes.*

Mr. Davis' *Geometrical Note* in the May *Gazette* (p. 110) gives me an occasion to repeat a favourite assertion, that work with oblique axes is brief if we use the projections l, m of a point as readily as the coordinates x, y. Need I say that in noticing how the analytical solution of this question illustrates my own point, I am not for a moment depreciating the geometrical solution?

The problem is, given two lines OX, OY and a point C, and a variable line PCQ cutting OX, OY in P, Q, to find the locus of the circumcentre of the triangle OPQ.

The feet of the perpendiculars from the circumcentre on OX, OY are the midpoints of OP, OQ; that is, if the projections of the circumcentre are l, m, and if $OP = p$ and $OQ = q$, then $p = 2l$, $q = 2m$. But because C is on PQ,

$$x_C/p + y_C/q = 1.$$

Hence the locus of the circumcentre is

$$x_C/2l + y_C/2m = 1,$$

a hyperbola which passes through the origin and has the lines $l=\frac{1}{2}x_c$, $m=\frac{1}{2}y_c$ for its asymptotes. If CF is parallel to OY and cuts OX in F, the equation $l=\frac{1}{2}x_c$ *expresses directly* that the foot of the perpendicular from (l, m) on OX is the midpoint of OF; that is, this line is the perpendicular bisector of OF; similarly $m=\frac{1}{2}y_c$ is the perpendicular bisector of OG, where G is in OY and CG is parallel to OX. E. H. N.

765. [V. 1. a. ζ.] *Note on the Formula* $AB.CD+BC.AD+CA.BD=0$.

1. If A, B, C, D are points on a straight line and a, b, c, d their distances from a given collinear point, this may be written

$$(a-b)(c-d)+(b-c)(a-d)+(c-a)(b-d)=0. \quad \ldots\ldots\ldots\ldots(1)$$

Formula (1) holds when a, b, c, d obey the combination laws of ordinary algebra.

Invert figure from any point, and we have Ptolemy's theorem.

2. In formula (1) now regard a, b, c, d as points in a plane or a flat three-space; then $a-b$, etc., represent vectors, and (1) still holds if we take the multiplication therein as scalar, since scalar multiplication satisfies the ordinary combination laws. But the scalar product of two vectors is zero when they are perpendicular. Hence:

If two pairs of opposite edges of a tetrahedron are perpendicular, so likewise is the third pair.

If we take a, b, c, d as coplanar, we have orthocentric property of the triangle.

3. Another interpretation, with Gauss' complex multiplication gives: The three mid-points of the diagonals of a complete quadrilateral are collinear. See Professor Genese's article in *Gazette*, May 1923, p. 300, Ex. 15.

4. Again take a, b, c, d as lines in a plane, and let ab represent the cut of a, b. If the homogeneous coordinates of a, b be respectively (l_1, m_1, n_1) and (l_2, m_2, n_2), then $(l_1m_2-l_2m_1, \ldots)$ are the coordinates of ab. We can thus regard ab as the vector product of a and b. Similarly if x, y be points, the join xy is the vector product of x and y. If points (x_1, y_1, z_1) and (x_2, y_2, z_2) are conjugate for a conic, which we can take as $x^2+y^2+z^2=0$, then

$$x_1x_2+y_1y_2+z_1z_2=0,$$

which we shall write $x \mid y=0$, where x, y are the points (x_1, y_1, z_1) and (x_2, y_2, z_2) respectively. [Scalar product.]

But $$ab \mid cd=(a \mid c)(b \mid d)-(b \mid c)(a \mid d);$$

$$\therefore\ ab \mid cd+bc \mid ad+ca \mid bd=0. \quad \ldots\ldots\ldots\ldots(2)$$

Hence we have Hesse's theorem. If two pairs of opposite vertices of a complete quadrilateral are conjugate w.r. to a conic, so likewise is the third pair.

The analogous interpretation of (2) in three-space merely gives the reciprocal property of the pole and polar w.r. to a quadric. The geometrical connection between Hesse's theorem and the property of the tetrahedron mentioned in § 2 is clear when we consider the cuts of the edges of the tetrahedron and the plane at infinity.

5. Other results which follow from the method of § 4, essentially due to Grassmann:—

In the plane: $$ab \mid c=(a \mid c)b-(b \mid c)a;$$

$$\therefore\ ab \mid c+bc \mid a+ca \mid b=0.$$

This gives (1) the reciprocal property of pole and polar w.r. to a conic. (2) Polar triangles are in perspective.

In space: $$abc \mid d=(a \mid d)bc+(b \mid d)ca+(c \mid d)ab;$$

$$\therefore\ abc \mid d-bcd \mid a+cda \mid b-dab \mid c=0.$$

This gives: If two tetrahedra be polar w.r. to a quadric, the joins of corresponding vertices lie on a regulus. H. G. Forder.

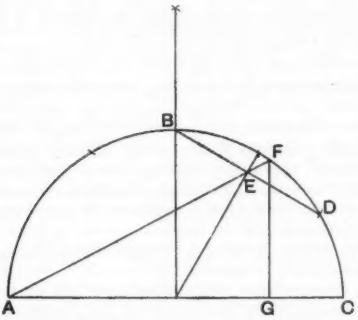
766. [X. 4. a.] Wallich's construction for $\sqrt{\pi}$ was given in his *Popular Tables*, and was communicated to the Royal Society by Prof. Stokes, 10/5/55.

It is substantially as follows:

AB a quadrant, radius unity. BD chord of 60° bisected at E.

Chord $AEF = 1{\cdot}771981$, $\sqrt{\pi} = 1{\cdot}772454$.

Defect ($\cdot 000473$) = 1/3750 of itself (app.).



Wallich's decimals have been cut down from 10 to 6 places.

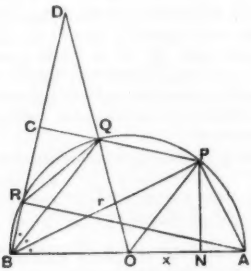
Note further that if FG be drawn perp. to AC,

$$AC \cdot AG = AF^2$$
$$= \pi \text{ (rad.)}^2 \text{ app.};$$
$$\therefore \ AG = \text{arc } AB \text{ app.},$$

which is true for all values of the radius to the same degree of approximation, the relative error being about 1/1875. If rad. = 2, $AG = \pi$.

E. M. LANGLEY.

767. [K¹. 21. b.] AP, PQ, QR are three consecutive sides of a reg. 7-gon. AOB the diameter through A.



Produce BR to meet PQ, OQ in C, D, then BCD is perpr. to PQ. Draw PN perpr. to OA.

Also $$BOD = OBD = \frac{3\pi}{7};$$

$$\therefore\ OD = BD$$

and
$$B\hat{D}O = \frac{\pi}{7} = Q\hat{B}D;$$

$$\therefore\ BC = CD$$

and
$$QD = BQ = 2ON.$$

Let
$$BP = r,\quad ON = x,\quad OA = a.$$

From
$$BP^2 = BA \,.\, BN \quad \text{and} \quad AB \,.\, BC = BP \,.\, BQ,$$

we easily find
$$r^3 - ar^2 - 2a^2r + a^3 = 0.$$

Put
$$r = \frac{2\sqrt{7}}{3}\, a \cos\phi + \frac{a}{3},$$

$$\frac{56\sqrt{7}}{27}\cos^3\phi - \frac{14\sqrt{7}}{9}\cos\phi + \frac{7}{27} = 0,$$

$$4\cos^3\phi - 3\cos\phi + \frac{1}{2\sqrt{7}} = 0,$$

$$\cos 3\phi = -\frac{1}{2\sqrt{7}},$$

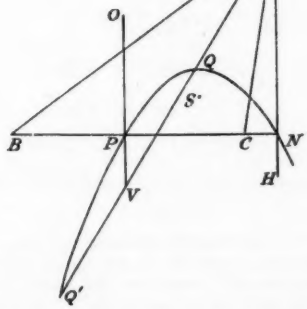
Hence 3ϕ is the angle BCG (p. 21) which Major Chepmell trisects by a neat use of the elliptic compasses for effecting the old Greek construction.

E. M. LANGLEY.

768. [K¹. 2. b, c.] *Proof of Feuerbach's Theorem.*

In the figure Q is the incentre, S is the nine-points centre, O the circumcentre and V is the middle point of the arc BC of circumcircle.

$$QV^2 = CV^2 = 2R \,.\, PV = 4SP \,.\, PV. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1)$$



If a parabola be described with focus S, axis parallel to PV, and passing through P (with vertex above P), then QV, which bisects the angle between the directions OA and PV, *i.e.* SP and PV, is parallel to the tangent at P.

Hence the point Q is on the curve, from (1).

The directrix is above BC, a distance $= SP = R/2$; and Q is distance r above BC; $\therefore\ SQ =$ distance of Q below the directrix $= R/2 - r$.

Again Q', the excentre, opposite A, is also on the curve, and similarly $SQ' = R/2 + r_1$. The parabola also passes through N. N. M. GIBBINS.

769. [L². 4. a.] *To find the lengths of the axes of a plane section of a non-central conicoid.*

The method indicated in Bell's *Coordinate Geometry of Three Dimensions*, p. 137, edition 1923, involves considerable labour, and the arithmetic may be considerably eased by the method illustrated in the following example:

"To find the lengths of the axes of the section of $2x^2+y^2=z$, by the plane $x+2y+z=4$."

The curve of intersection also lies on the cylinder

$$2x^2+y^2+x+2y=4$$

or

$$2(x+\tfrac{1}{4})^2+(y+1)^2=\tfrac{41}{8}.$$

All parallel planes cut a cylinder in equal sections, and the problem is thus reduced to that of finding the lengths of the axes of $2x^2+y^2=\frac{41}{8}$ by

$$x+2y+z=0.$$

If r be the length of a semi-axis, we have therefore

$$\begin{vmatrix} \frac{16}{41}-\frac{1}{r^2} & 0 & 0 & 1 \\ 0 & \frac{8}{41}-\frac{1}{r^2} & 0 & 2 \\ 0 & 0 & -\frac{1}{r^2} & 1 \\ 1 & 2 & 1 & 0 \end{vmatrix}=0$$

or

$$\left(\frac{16}{41}-\frac{1}{r^2}\right)\left(\frac{8}{41}-\frac{1}{r^2}\right)+\left(\frac{16}{41}-\frac{1}{r^2}\right)\frac{4}{r^2}+\left(\frac{8}{41}-\frac{1}{r^2}\right)\frac{1}{r^2}=0,$$

which leads to

$$-\frac{6}{r^4}+\frac{96}{41}\cdot\frac{1}{r^2}-\frac{128}{r^4}=0,$$

or

$$\left(\frac{8}{41}r^2\right)^2-6\left(\frac{8}{41}r^2\right)+3=0,$$

$$\frac{8}{41}r^2=5\cdot4495 \quad \text{or} \quad 0\cdot5505,$$

$$r=5\cdot28 \quad \text{or} \quad 1\cdot68.$$

The coordinates of the centre are $(-\frac{1}{4}, -1, 6\frac{1}{4})$, the z value being obtained by substitution of those of x and y in the equation of the plane. The directions of the axes can be obtained by considering those of the section of

$$2x^2+y^2=\tfrac{41}{8},$$

by the plane $x+2y+z=0$, in the usual manner.

The method can clearly be applied to any numerical case of the conicoid, however placed it may be with regard to the axes of reference (which are taken to be rectangular with equal base units). The cylinder is of the type $ax^2+by^2+2hxy+2gx+2fy+c=0$ (which referred to parallel axes, with the line of centres as axis of x, becomes $ax^2+by^2+2hxy+\frac{D}{C}=0$), and the complication thus entailed in the determinant consists of the replacement of each of two zeros by the expression $-\frac{Ch}{D}$ derived from the equation of the cylinder.

University College, Nottingham,
Oct. 29, 1924.

H. G. GREEN.

770. [L¹. 4.] *The equation of the tangent at a point to a curve in trilinear or areal coordinates.*

Suppose X, Y, Z are the current coordinates, $U=0$ the equation of the curve, and x, y, z the point on the curve.

Then the equation of the tangent is:

$$\begin{vmatrix} X & Y & Z \\ x & y & z \\ x+\delta x & y+\delta y & z+\delta z \end{vmatrix} = 0;$$

i.e.
$$\begin{vmatrix} X & Y & Z \\ x & y & z \\ \delta x & \delta y & \delta z \end{vmatrix} = 0.$$

Hence
$$\begin{vmatrix} X\dfrac{\partial u}{\partial x} & Y\dfrac{\partial u}{\partial y} & Z\dfrac{\partial u}{\partial z} \\ x\dfrac{\partial u}{\partial x} & y\dfrac{\partial u}{\partial y} & z\dfrac{\partial u}{\partial z} \\ \dfrac{\partial u}{\partial x}\partial x & \dfrac{\partial u}{\partial y}\partial y & \dfrac{\partial u}{\partial z}\partial z \end{vmatrix} = 0;$$

Adding the other columns to the first,

$$\begin{vmatrix} \Sigma X\dfrac{du}{dx} & Y\dfrac{\partial u}{\partial y} & Z\dfrac{\partial u}{\partial z} \\ \Sigma x\dfrac{\partial u}{\partial x} & y\dfrac{\partial u}{\partial y} & z\dfrac{\partial u}{\partial z} \\ \Sigma\dfrac{\partial u}{\partial x}\partial x & \dfrac{\partial u}{\partial y}\partial y & \dfrac{\partial u}{\partial z}\partial z \end{vmatrix} = 0,$$

i.e.
$$\begin{vmatrix} \Sigma X\dfrac{\partial u}{\partial x} & Y\dfrac{\partial u}{\partial y} & Z\dfrac{\partial u}{\partial z} \\ 0 & y\dfrac{\partial u}{\partial y} & z\dfrac{\partial u}{\partial z} \\ 0 & \dfrac{\partial u}{\partial y}\partial y & \dfrac{\partial u}{\partial z}\partial z \end{vmatrix} = 0;$$

$$\therefore\ \Sigma X\frac{\partial u}{\partial x}=0 \quad \text{or} \quad y\,\delta z - z\,\delta y = 0.$$

If $y\,\delta z - z\,\delta y = 0$, it is clear from the relations

$$\frac{\partial u}{\partial x}\partial x+\frac{\partial u}{\partial y}\partial y+\frac{\partial u}{\partial z}\partial z=0 \quad \text{and} \quad x\frac{\partial u}{\partial x}+y\frac{\partial u}{\partial y}+z\frac{\partial u}{\partial z}=0$$

that $\dfrac{\partial u}{\partial x}=0$.

Thus the possible relation $y\,\partial z - z\,\partial y = 0$ is introduced when multiplying by $\dfrac{\partial u}{\partial x}$. Similarly for the others.

Hence the equation of the tangent is $X\dfrac{\partial u}{\partial x}+Y\dfrac{\partial u}{\partial y}+Z\dfrac{\partial u}{\partial z}=0$.

A. W. Lowry.

771. [K¹. 6. a.] *The Axes being oblique, to find the Angle between two directions.*

Let the ratios (x_1, y_1, z_1), (x_2, y_2, z_2) be given by the equations

$$T \equiv \Sigma ux = 0, \quad \text{.....(1)}$$

$$S \equiv \Sigma ax^2 + 2\Sigma fyz = 0. \quad \text{.....(2)}$$

NOTATION.

α, β, γ are the angles of the given frame, and θ the required angle.

Δ, ϕ, Δ' are the coefficients of k^2, k, k^0 in the equation

$$\begin{vmatrix} ka+1 & kh+\cos\gamma & kg+\cos\beta & u \\ kh+\cos\gamma & kb+1 & kf+\cos\alpha & v \\ kg+\cos\beta & kf+\cos\alpha & kc+1 & w \\ u & v & w & 0 \end{vmatrix} = 0.$$

$$S_{pq} \equiv \Sigma a x_p x_q + \Sigma f(y_p z_q + y_q z_p),$$

$$\sigma \equiv \Sigma x^2 + 2\Sigma yz \cos\alpha,$$

$$\sigma_{pq} \equiv \Sigma x_p x_q + \Sigma(y_p z_q + y_q z_p)\cos\alpha.$$

First solution.

The values of k for which the equations

$$\left.\begin{aligned} T &= 0, \\ kS + \sigma &= 0, \end{aligned}\right\} \quad \text{.....(3)}$$

give coincident directions are the roots of the equation

$$k^2\Delta + k\phi + \Delta' = 0. \quad \text{.....(4)}$$

Now, if the directions of reference be changed, and the directions given by (1) and (2) be taken for $y=0$, $x=0$; and $z=0$, $x=0$; and if θ, $\pi/2$, $\pi/2$ be the angles of the new frame, the equations (3) are transformed into

$$x=0 \quad \text{and} \quad k(a_1x^2 + 2\Sigma f_1 yz) + \Sigma x^2 + 2\Sigma yz\cos\theta = 0.$$

The equation (4) therefore becomes

$$k^2 f_1^{\,2} + 2kf_1\cos\theta - \sin^2\theta = 0; \quad \text{.....(5)}$$

$$\therefore\ f_1^{\,2}/\Delta = (2f_1\cos\theta)/\phi = -(\sin^2\theta)/\Delta';$$

$$\therefore\ \tan^2\theta = -4\Delta\Delta'/\phi^2.$$

Second solution.

From the usual formula

$$\cos^2\theta = \sigma_{12}^{\,2}/\sigma_{11}\sigma_{22},$$

we get

$$\tan^2\theta = (\sigma_{11}\sigma_{22} - \sigma_{12}^{\,2})/\sigma_{11}\sigma_{22}. \quad \text{.....(6)}$$

Now the ratios $(\lambda x_1 + x_2,\ \lambda y_1 + y_2,\ \lambda z_1 + z_2)$ satisfy the equation $T=0$ for all values of λ. Hence, remembering that $S_{11} = S_{22} = 0$, the values of k for which the equation $T=0$ and $kS+\sigma=0$ give coincident directions are the roots of the equation

$$k^2 S_{12}^{\,2} + 2kS_{12}\sigma_{12} - (\sigma_{11}\sigma_{22} - \sigma_{12}^{\,2}) = 0.$$

They are also the roots of the equation

$$k^2\Delta + k\phi + \Delta' = 0;$$

$$\therefore\ S_{12}^{\,2}/\Delta = 2S_{12}\sigma_{12}/\phi = -(\sigma_{11}\sigma_{22} - \sigma_{12}^{\,2})/\Delta';$$

$$\therefore\ \tan^2\theta = -4\Delta\Delta'/\phi^2.$$

Either method is applicable to the problem of calculating the angle between two vectors whose coefficients satisfy simultaneously $(n-2)$ linear equations and a homogeneous quadratic. The value of $\tan^2\theta$ will be found to be $-4DD'/\psi^2$, where D, D', ψ are the coefficients of a certain quadratic equation.

Ceylon University College, Colombo. F. H. V. GULASEKHARAM.

REVIEWS.

The Mathematical Theory of Relativity. By A. S. EDDINGTON. 2nd edition. Pp. ix + 270. 20s. net. 1924. (Cam. Univ. Press.)

Einstein's *Generalized Theory of Relativity* was published during the war, and the only accounts accessible in England were three papers by Prof. de Sitter in the *Monthly Notices of the Royal Astronomical Society*. Near the end of 1918 a *Report on the Relativity Theory of Gravitation* by Prof. Eddington was published by the Physical Society. In this small book of 90 pages an admirable exposition was given of this difficult and novel theory. This report was succeeded by *Space, Time and Gravitation*, in which the subject was presented as far as possible in popular language. In this work the lucid exposition of a grave subject is enlivened by so much that one need not wonder that already four impressions have been issued. The present work, which has now reached a second edition, is intended for the mathematical student who has derived from the less technical book a general grasp of the revolution of thought associated with the theory of relativity.

The book is not easy reading, but no one would expect it to be. It is, however, enlivened by occasional flashes of wit, which are all the more amusing from their sudden appearance in a grave argument.

Prof. Eddington, in a short introductory chapter, explains that he is writing a treatise on physics, not on pure mathematics. *A physical quantity is defined by the series of operations and calculations of which it is the result.* By the study of physical measurements we obtain our knowledge of the condition of the world. The mathematical theory of tensors enables us to express simultaneously the group of measurements associated with any condition of the world in the same form, whatever system of coordinates have been used in making the measurements.

The first chapter begins by assuming that the square of the interval between two near events may be expressed as a quadratic function of four coordinates. When the coefficients are constant this can be transformed into the sum of four squares, three of which are spatial and one temporal. The Lorentz transformation leaves the form of this expression unchanged, and the "restricted" theory of relativity is developed. The velocity of light is the same for all observers, the change of mass with velocity deduced, and agreement found with the optical experiments of Fizeau and Michelson.

In the second chapter the Tensor Calculus is developed. A careful explanation is given of what is implied in the terms vector and tensor. The mathematical theory is made as easy as it can be made. The reader will not be long before he is able to follow Prof. Eddington, though in most cases a good deal of time and study will be required before he is at home in this fascinating but difficult branch of mathematics. The description of certain symbols as "dummies" is a great help over one of the difficulties.

In the third chapter we come to Einstein's Law of Gravitation. The vanishing of the Riemann-Christoffel tensor is shown to be the necessary and sufficient condition for flat space-time, *i.e.* for the Euclidean geometry and time of the "restricted" theory of relativity. The vanishing of the "contracted" Riemann-Christoffel tensor in empty space is assumed by Einstein for his law of gravitation. The application of this law to an isolated particle —afterwards taken as the Sun—leads to the three famous experiments or observations which have vindicated the law, viz. the movement of the perihelion of Mercury, the bending of light verified in solar eclipses, and the displacement to the red of the lines in the solar spectrum. The second edition was apparently in print before the complete verification of these predictions of Einstein had been made. The eclipse observations of English and American observers and the spectroscopic observations of Evershed and St. John leave little doubt of the correctness of Einstein's law.

The fourth chapter deals with mechanics. A tensor is found, called the energy tensor, closely related to the tensor whose vanishing in free space determined the law of gravitation. We find, with some astonishment, that the equations of hydrodynamics are deduced at once from the properties of this tensor.

The fifth chapter deals with the curvature of space and time. A slight modification of Einstein's original law leads to the possibility of a finite universe, analogous to the surface of a sphere as compared with a plane in two-dimensional geometry. This is attractive, because then the radiation of the stars would be conserved. Different pictures of such a universe have been painted by Einstein and de Sitter. Effects might be shown in the movements of the most distant bodies with which astronomers are acquainted, the spiral nebulae, if their distances are comparable with the radius of this spherical world imagined by de Sitter. Prof. Eddington brings together the astronomical evidence, but no certain conclusion can be drawn. The subject is extremely speculative.

The sixth chapter deals with electricity, and it is shown how readily the tensor-calculus deals with problems of the electromagnetic field.

In the last chapter an account is given of Weyl's *World Geometry*, which the author describes as unquestionably the greatest advance in the relativity theory after Einstein's work. In the second part of this chapter he gives an account of his own researches, in which he has shown that Einstein's fundamental tensor is divisible into two parts, one of which describes the geometrical and mechanical properties, and the other the electro-magnetic properties of the world.

The reviewer has learnt much from this book, but cannot profess to have assimilated it at all completely. This will be found difficult by many readers. But Prof. Eddington by recapitulations and summaries has done what is possible to make clear questions of great generality which demand a good deal of thought. All mathematicians who are interested in the theory of relativity will learn much from this very fine presentation of the subject.

The Observatory, Greenwich. J. W. DYSON.

Statics, including Hydrostatics and the Elements of the Theory of Elasticity. By H. LAMB. New and Revised Edition. Pp. xii + 357. 12s. 6d. 1924. (Cambridge University Press.)

Students of Applied Mathematics, to whom it is of great importance to have text-books dealing with the principles of Mechanics, have learnt to be grateful to Professor Horace Lamb for the excellent books he has written on the subject. This is abundantly proved by the manner in which new editions of Professor Lamb's books are called for. The volumes on *Statics*, *Dynamics* and *Higher Mechanics* have become recognised companions for all students in our Honours Schools at the Universities, while the monumental *Hydrodynamics* has become recognised inside and outside this country as the standard work on the subject.

The present volume on *Statics* was first published in 1912. It was reprinted in 1916 and 1921, and now we have a fourth issue in the form of a second edition. There are obvious reasons why Professor Lamb's *Statics* should enjoy this vogue. The contents are eminently suitable. The book deals with Statics in the ordinary sense for about two hundred pages, and then about a hundred and fifty pages are devoted to Hydrostatics and to the elementary theory of Elasticity. In each case the topics chosen for treatment are just those most required by the student, and the treatment is clear and interesting. This applies particularly to the Hydrostatics and to the Elasticity.

In the case of the Statics proper, there is considerable room for doubt as to whether the arrangement is all that is to be desired. There is a certain lack of system, and where the student is not already familiar with all the principles, he would find it very difficult to *learn* the subject from the present book. Further, the proportions of the spaces allowed to the different topics in the *Statics* call for some criticism. One is, of course, aware of the fact that in the older books on Statics, authors were apt to devote an excessive amount of attention to the purely theoretical and merely manipulative aspects of the subject, with the result that matters like Chains would enjoy long and detailed treatment quite out of keeping with their practical importance. Perhaps, however, Professor Lamb has gone too much to the other extreme, and thirteen pages on Chains seem quite inadequate in comparison with twenty-

six pages on Mass Systems, or an equal amount of space devoted to a rather theoretical discussion of Frameworks and Graphical Methods.

The new edition does not differ from the first in the parts dealing with Statics proper and with Hydrostatics. The section on Elasticity has been considerably altered. New material has been included, as, for example, on the Principle of Least Energy, on the Flexure of a Curved Bar, and on the Theory of the Rotating Shaft. The treatment has also been touched up in respect to many details, and several new examples added. It is worth noticing also that a new set of twenty-six miscellaneous examples has been added. The return to a more reasonable price is another noticeable feature.

The book has thus been made more useful and more welcome, especially when used in conjunction with judicious teaching and direction. The criticisms that have been offered are inspired by a desire to see a really first-class text-book put in the hands of the British university student. There can be no doubt that Professor Lamb's book on Statics approaches this ideal more closely than any treatise in the English language. S. B.

Binomial Factorisations. By Lt.-Col. A. J. C. Cunningham, R.E. Vol. II. pp. 1 xxix+215. 15s. Vol. VI. pp. 103. 5s. 1924. (London, Francis Hodgson.)

These two volumes give, in the first place, tables showing what integral values of y satisfy the congruences

$$\frac{y^5-1}{y-1}\equiv 0, \quad \frac{y^{10}+1}{y^2+1}\equiv 0, \quad \frac{(y^{15}-1)(y-1)}{(y^3-1)(y^5-1)}\equiv 0 \pmod{p},$$

and some others, for extensive ranges of values of the prime modulus p. From these tables there are obtained the prime factors of $y^5\pm 1$, $y^{10}+1$, etc., for wide ranges of values of y. Thus all factors not exceeding 100,000 of $(y^{10}+1)/(y^2+1)$ up to $y=200$ are recorded, the resolution being complete when $y<45$, and for certain greater values such as $y=71,133,170$. Aurifeuille's identity,

$$f(x, y)=x^4+x^3y+x^2y^2+xy^3+y^4=(x^2+3xy+y^2)^2-5xy(x+y)^2,$$

leads to a resolution of $f(x, y)$ whenever $5xy$ is a rational square. This and similar identities have been made the basis of detailed factor-tables by Lt.-Col. Cunningham, other results being given in Vol. I. (noticed in the issue of July 1924). Three more volumes are announced to appear in the next few months, and further criticism is reserved until the set is complete.

W. E. H. B.

The Direct Numerical Calculation of Elliptic Functions and Integrals. By L. V. King. Pp. viii+42. 3s. 6d. net. 1924. (Cam. Univ. Press.)

The calculation of the numerical values of the Elliptic Integrals is a work that one would always prefer, if possible, to get someone else to do for one. The reason for reluctance to undertake it is that, though it is possible to obtain series for the trigonometrical functions of $x\left(=\frac{\pi u}{2K}\right)$ in terms of powers of the elliptic functions sn u, cn u or dn u, and so to determine u from the values of sin ϕ or cos ϕ, yet these series are very cumbersome, not quickly convergent, and require elaborate preliminary calculations of the q-functions which arise in the coefficients. One is therefore practically forced to determine u from ϕ by means of the repeated use of Landen's transformation with its inevitable recurrence formulae. The latter are distasteful to the computer who prides himself on accuracy, because errors accumulate. The trouble is not with the setting out of the scale of arithmetico-geometric means, which is so rapid that for a large modular angle of 85° only five steps are needed to ensure accuracy to sixteen decimal places at least. It is on account of the recurrence formulae

$$\tan(\phi_{n+1}-\phi_n)=\frac{b_n}{a_n}\tan\phi_n,$$

$$\sin(2\phi_{n-1}-\phi_n)=\frac{c_n}{a_n}\sin\phi_n.$$

Taken as they stand, they necessitate repeated extractions from trigonometrical tables of the angles corresponding to given tangents or sines and *vice versâ*, with great opportunity for error, especially when the tangents are large. It is true that the first of these formulae can be put in the form

$$\tan \phi_{n+1} = \frac{\left(1 + \frac{b_n}{a_n}\right) \tan \phi_n}{1 - \frac{b_n}{a_n} \tan^2 \phi_n},$$

which lends itself to machine computation, but the second leads to a quadratic equation for $\sin \phi_{n-1}$ and is not so easily adapted.

Since there is practically no escape from these recurrence formulae when the elliptic argument has to be obtained from the amplitude or *vice versâ*, we are indebted to Dr. King for his extremely useful and concise account of the process, and for a valuable collection of new formulae. He is particularly to be thanked for his method of overcoming the difficulty of evaluating the theta functions of a complex argument required in connection with the third Elliptic Integral in its circular form—a form which nearly always presents itself in dynamical problems. His method is greatly superior to that given by Somoff in *Crelle's Journal* in 1854.

Among the many new formulae given in the book are those for increasing or decreasing hyperbolic amplitudes. These are apparently chiefly useful in determining the integrals and functions involving the complementary modulus, and therefore aiding in the computation of their values for imaginary arguments. As the formation of the arithmetico-geometric scale is so simple and expeditious, it is not obvious what advantage the introduction of the hyperbolic functions has over the recompiling of the arithmetico-geometric scale for the complementary modulus, especially as tables of the hyperbolic functions are less accessible and less minutely computed than those of the circular functions.

Fortunately, however, there is a large number of dynamical and kinematical problems in which the determination of the argument from the amplitude (or *vice-versâ*) is unnecessary. In these, the elliptic functions of known fractions of the period are required which would seem to be most easily obtained from the familiar q-series for $\operatorname{sn} u$, $\operatorname{cn} u$, $\operatorname{dn} u$ and Zu. Dr. King's methods of finding the nome q and the period K are very simple and expeditious for any modulus. The nome q has, however, been calculated to 20 places of decimals for every degree of the modular angle θ, and K to 10 places for the same intervals. Dr. A. L. Baker has given a highly convergent series for q in terms of powers of $\tan^2 \frac{1}{2}\theta$ in his *Elliptic Functions* (p. 95), the first eight coefficients of which have been worked out. There is little probability of error in its use, because the process of forming the powers of $\tan^2 \frac{1}{2}\theta$ is so easily checked, step by step. It has the advantage, when great accuracy is desired, of not requiring antilogarithms to be computed. It may be observed in passing that it is never necessary to calculate q from this formula for values of θ greater than 23°; for if q_1 be calculated for $k_1 = \frac{1 - k'}{1 + k'}$, $q = \sqrt{q_1}$, etc.

Graphical methods have been invented for the purpose of determining the numerical values of the elliptic integrals. Machines for the same evaluations are in existence. One was constructed by M. Delaunay and described by him in the *Bulletin des Sciences mathématiques* in 1902. It gave the value of the first elliptic integral and was subsequently adapted for the second elliptic integral from a hint given by one of his pupils, M. Lipetz. Another instrument was made by Mr. Ravenshear and formed the subject of a communication to the Physical Society in November 1915. His machine very cleverly describes graphs of the integrand in the case of each of the three elliptic integrals, from which the integrals themselves are determined by means of a planimeter or by measurement of the ordinates. NEMO.

A Companion to Elementary School Mathematics. By F. C. BOON. Pp. 302. 14s. net. 1924. (Longmans, Green and Co.)

This is a book which can scarcely fail to be useful to all mathematical teachers except those whose work is confined to teaching the very lowest forms.

It begins with Biographical Notes on those Mathematicians who have contributed to what is now elementary work and an eight-page historical outline.

This is followed by twenty chapters, each dealing with a portion of the subject, which lends itself to a discursive treatment usually linking pieces of work which are liable to be kept unduly separated in the pupil's mind. The headings of some of the chapters—Squaring the Circle; Nomenclature; Symmetry; Analogy; Limits; Converses; Induction; Paradoxes and Fallacies—will indicate the wide range of the book.

The chapters are not all of equal interest, but the teacher may turn to any of them confident of finding in it the material for an interesting lesson (or in some cases two or three lessons) suitable for use when the grind through the set syllabus can be varied.

Or a boy of a moderate Fifth Form standard could be given a chapter or two to read to fill in an odd hour.

On the whole a most valuable book to keep somewhere handy to the teacher's desk.

C. O. T.

Petit Traité de Perspective. By R. BRICARD. Pp. 83. n.p. 1924. (Libraire Vuibert.)

As to the aim and scope of this treatise of eighty-three pages, we cannot do better than quote from its preface: "En écrivant ce *Petit traité de perspective* je n'ai jamais perdu de vue le titre qui j'entendais lui donner et j'ai fui la prolixité. J'avais surtout pour but de répandre la méthode dont Cousinery a indiqué le principe en 1828 dans sa *Géométrie perspective* et que l'on méconnait en France . . . Quand on expose la perspective devant un auditoire suffisament instruit, il convient de la rattacher aux idées générales de la Géométrie projective. Mais désirant que ce livre fût accessible au plus grand nombre possible de lecteurs, je n'y ai mis de Géométrie projective que l'indispensable, les notions de point à l'infini, de droite à l'infini d'un plan, de plan à l'infini."

Taking the nine chapters in order:

I. explains the use of elements at infinity and gives a brief but lucid account of plane homology. As this must be intended for those readers who are not sufficiently instructed in the general ideas of projective geometry, the insertion, in the vacant space on its last page, of some simple exercises would add greatly to its value. Among these the case of two circles, homologous in two different ways, would be helpful.

II. explains the object of perspective and its physical and psychological principles. It is clear, interesting and suggestive.

III. deals with the geometrical principles of perspective, giving concisely the requisite definitions and theorems. A contrast is drawn between "Perspective independante" (in which the constructions in the picture plane are based on elements given directly in perspective), and "Perspective appliquée" (in which they depend on elements obtained by orthogonal projection in "plan" and "elevation"). The author is a strong advocate for the former ("Brook Taylor's," or, as he would say, "Cousinery's").

IV., after explaining how to obtain in the picture plane the fundamental elements on which the constructions in it are based, and pointing out the two classes, descriptive and metric, into which problems in general fall, gives solutions of four descriptive and one semi-metric.

V. is devoted to the solution of metric problems, ranging from such simple ones as rabatting the eye into the picture plane about a line in it, finding the angle between two given straight lines, to those of finding the perspective of a sphere and its shadow. Here again a set of examples, of gradually increasing difficulty, accompanied by hints for solution, based on the fundamental problems already worked, would have been a useful addition and might have profitably filled up the blank on p. 47.

VII. and VIII. deal respectively with the direct and inverse problems of applied perspective, and have the same merits and lucidity as V. and VI. The author draws attention to the increased inportance of the study of the inverse problem owing to the rise of *Metrophotography* since the introduction of reconnaissance and survey by aeroplanes. Admitting his plea of want of space, we should have welcomed the insertion of a small section such as that devoted to

Perspective Plotting, § 267, and the Four-point Method, § 268, of the *Manual of Map Reading and Field Sketching*, 1921. He gives, however, useful references to works by Saconney (Doin), D'Ocagne and Schilling (Gauthier Villars).

VIII. discusses the process of *mental restitution* and its bearing on the question as to how far an artist may profitably deviate from the strict laws of geometric perspective. A brief historical résumé of the art is given, interesting as far as it goes; a reference to the articles contributed by G. Loria in "Perspective and Descriptive Geometry," to Vol. IV. of Cantor's *Geschichte der Mathematik*, pp. 579-637, might have been useful to readers wishing to go more deeply into the subject.

IX. treats of Cavalier Perspective (or Cylindrical Projection). This is welcome, for it is the method generally adopted, consciously or unconsciously, and more or less exactly, by authors of text-books, and is worthy of more attention than they appear to give to the explanation of its principles. We may hope that the subject may be included in those "informal lessons . . . to be illustrated by models and experimental demonstrations recommended in the recent report issued by the M.A. The work concluded with a set of twenty exercises and a note on authors recommended to students desirous of pursuing the subject further. E. M. L.

Mensuration for Middle Forms. By H. W. CARTER. Pp. 110. 1s. 6d. 1924. (George G. Harrap and Co.)

This useful little book includes calculations from field-book readings, and covers the ground up to pyramids, right circular cones, and spheres, and their frusta. It does not give Simpson's Rule, though that includes all the formulae for the solids considered. These are 4-figure tables of logarithms and anti-logarithms for use with the book, so arranged that they can be read wherever the book may be open: a great convenience. Reasons are given for each of the formulae used, to prevent blind acceptance of such formulae.

On p. 26 it is a pity that columns were not used in determining $s-a$, $s-b$, $s-c$ from values of a, b, c, and the check explained that their sum should equal s. The best way of using logarithms is well indicated. For them, columns are essential.

As a rule the diagrams are good, but on p. 57, Fig. 36 needs redrawing. The rectangular slab in perspective actually has its further parts larger than the nearer. And on p. 83 the radius of the sector should have been drawn equal to the slant height of the cone.

The only graphic determination of areas suggested is that of reducing a polygon to a triangle. More, perhaps, might have been given usefully, such, *e.g.* as the reduction of a quadrilateral or triangle to an equivalent triangle of some convenient height such as 2 inches, in which case the length of base in inches = the number of square inches in the area. Graphic determination is a useful check on calculation. This, however, could be supplied by the teacher if he thinks fit.

There are no answers given, but possibly copies with answers would be supplied for the teacher.

The examples are numerous and varied and excellent.

Surveying for Schools and Scouts. By W. A. RICHARDSON. Second edition. Pp. i-vi+1-110. 2s. 6d. 1914. (George Philip and Son.)

This comprehensive little book deals with the various methods of surveying and traversing, by chain, compass, and plane table—also determination of levels and plotting of contours. It does not, of course, do away with the need of a competent instructor, but would enable the pupil to carry out his work intelligently. The explanations are very clear, and given in simple language, and the book is evidently written by an expert in teaching as well as in surveying. Rough traversing by pacing, with the compass, is well explained, and would be very useful in scout work in the country.

It is not clear why, p. 45, the dial should have its zero point directed to the various points included in the surveying instead of being clamped with its zero line pointing at the second station, which surely would be much simpler and far more speedy.

On p. 71 "if we divide the horizontal equivalent by the vertical, we obtain the *gradient*" is not very clear, though the next sentence does show what is intended, and the division is reversed a few lines further down.

On p. 74, Figs. 81, 82, *CD* is used in two different senses, which might be confusing.

On p. 89 (Fig. 99) the path of a ray from a point *C* to the eye-piece of the sextant is not very convincing.

These, however, are small blemishes in an admirable book.

It might be well, in Chapter XV. (pp. 63, 66), dealing with the Survey of Great Britain, to mention that in such an extended survey the curvature of the earth had to be taken into account, which is not necessary in the small surveys dealt with in the book. A. LODGE.

Obituary.

MR. JOHN MAXIMILIAN DYER died in January at Southbourne-on-Sea at the age of seventy-one. He was educated in Germany and at University College, London. He went up to Worcester College, Oxford, as a scholar, and obtained a second in Mathematical Moderations and a first in the final school of Mathematics in 1874. In 1877 he obtained the Senior University Mathematical Scholarship, and was appointed a master at Cheltenham College. There he remained eleven years, and in 1888 was appointed a master at Eton. He had a singular faculty for finding latent talent, and he was one of those who attracted attention to the History of Mathematics as a way of helping to realize the sequence of the development of ideas, before that plan was as generally accepted as it is now.

He had a great love of "elegant" solutions. It was a theory of his that for Mathematics "you need an eye, just as you do for cricket." He was the author of text-books on trigonometry and analytical geometry, and had a particular partiality for trilinears.

He was a member of the Mathematical Association, and on its Council for six years.

He retired from his Eton mastership in 1911; but, during the War, returned to active work in the Meteorological Department of the Admiralty.

V. LE NEVE FOSTER.

THE PILLORY.

London, B.Sc. Hons., 1923.

Prove that $\int f(x)/\phi(x)\,dx$, where $f(x)$ and $\phi(x)$ are polynomials in x, is in general the sum of a rational function of x and of the logarithm of a rational function (assuming that $\phi(x)$ has only *real* linear factors possibly repeated).

$\left[\text{Is } \frac{1}{2\sqrt{2}} \log \frac{x-\sqrt{2}}{x+\sqrt{2}} \text{ the logarithm of a rational function?}\right]$ E. H. N.

A QUERY.

Can any reader tell the Librarian who was the author of *A Syllabus of the Differential and Integral Calculus, Part I.*, printed by R. Harwood, Bridge Street, Cambridge, 1825, and whether any other parts were published?

ERRATA.

P. 271. Proof of Euc. I. 16, l. 9. *For* $E\hat{B}C$ *read* $E\hat{C}D$.

l. 11. *For* $E\hat{C}D$ *read* $A\hat{B}C$.

P. 275, l. 25 up. *For* vigour *read* rigour.

THE LIBRARY.

160 CASTLE HILL, READING.

ADDITIONS.

The Librarian reports gifts as follows:

From Prof. **R. W. Genese**, a third selection:

(1) The following books:

G. F. BAERMANN	Elementorum Euclidis Libri XV - - - - - An edition specially commended by Brunet.	1743
G. BOOLE	Differential Equations - - - - - - -	1859
W. E. BYERLY	Differential Calculus - - - - - - -	1882
R. CARMICHAEL	Calculus of Operations - - - - - - -	1855
S. EARNSHAW	Dynamics (2) - - - - - - - - -	1839
H. GODFRAY	Astronomy - - - - - - - - - -	1866
J. F. W. HERSCHEL	Preliminary Discourse on the Study of Natural Philosophy { } - - - - - - - - -	1851
C. HUTTON	Course of Mathematics { : W. Rutherford} - -	1849
W. W. JOHNSON	Differential Equations - - - - - - -	1889
D. LARDNER	Natural Philosophy: Hydrostatics, Pneumatics, and Heat - - - - - - - - -	1862
	Natural Philosophy: Electricity, Magnetism, and Acoustics - - - - - - - - -	1863
R. MURPHY	Electricity - - - - - - - - -	1833
P. NICHOLSON	Projection - - - - - - - - -	1840
J. M. PAGE	Ordinary Differential Equations - - - -	1897
G. SALMON	Conic Sections (2) - - - - - - - -	1850
E. H. SMART	Projective Geometry - - - - - - -	1913
H. J. S. SMITH	Report on the Theory of Numbers - - - Offprints, bound in one volume, of the six sections of this classical Report to the British Association.	1859-1865
B. WILLIAMSON	Elasticity - - - - - - - - -	1894

(2) Offprints by A. G. Greenhill and W. Wirtinger, and the following pamphlets:

H. AMSTEIN	*Etude élémentaire des courbes planes au moyen des coordonnées tangentielles.*	
J. H. POINCARÉ	*Sur les propriétés des fonctions définies par les équations aux différences partielles* - - - Poincaré's thèse de doctorat; Lebon gives the title incorrectly.	1879

(3) Two of the three *Rice Institute Pamphlets* devoted to mathematics, the *Reports* of the British Association for the years 1919-1923, and a run of the *Proceedings of the London Mathematical Society* from Ser. 2, Vol. 18 (1919-20) to date.

The Library would welcome volumes of the Proc. L.M.S. *between* 1887 *and* 1919.

(4) A further collection of school-books, by the following writers:

O. Airy; W. M. Baker, S. Barnard and J. M. Child, H. R. Birch, E. A. Bowser, F. G. Brabant, W. R. Browne;
A. G. Cracknell; C. Davison; E. J. Edwardes;
W. Garnett, P. Goyen, J. Graham, R. Graham, J. Greaves, E. J. Gross;
G. B. Halsted, R. Harris, F. Harrison, J. Harrison, C. Hawkins, R. S. Heath; W. H. Jackson, W. E. Johnson;
P. A. Lambert, W. H. Laverty, H. Law, D. D. Leib, J. Lightfoot, J. B. Lock (4), A. Lodge, T. Lund;
A. M. McNeile and J. D. McNeile, W. H. Miller; S. Newth; G. A. Osborne;
S. Parkinson (2), W. E. Paterson, R. H. Pinkerton, R. Potts;
G. Richardson, H. A. Roberts; C. A. Scott, J. H. Smith;
I. Todhunter (13), C. O. Tuckey; D. H. Vachha;
J. Wilson and S. R. Wilson, J. Wood (2), W. P. Workman and A. G. Cracknell (2), R. Wormell.

From Col. **R. L. Hippisley**:

L. V. King — Numerical Calculation of Elliptic Functions - - 1924

From Sir **Thomas Muir**:

The Mathematical Monthly. Vols. 1-3 - - - - - - 1858-61
Edited by J. D. Runkle and published at Cambridge, U.S.A. At the close of the third volume publication was "discontinued until further notice" "on account of the present disturbed state of public affairs," and not only was the journal one of the victims of the Civil War, but the fact that publication was never resumed is a sidelight on the havoc caused by the conflict. The *American Mathematical Monthly*, founded in 1894, was not a successor to Runkle's.

From Prof. **T. P. Nunn**, commemorating his term of office as President, 1917-1918:

E. H. Neville — Prolegomena to Analytical Geometry - - - 1922

From Mr. **C. W. Payne**:

M. Chasles — Sections Coniques; I - - - - - - - 1865
ALL PUBLISHED.

M. Faraday — Experimental Researches in Electricity (3 vols.) -
Quaritch's facsimile reprint, undated.

P. Frost — Curve Tracing - - - - - - - - 1872

[Lord Kelvin] W. Thomson [Lord Kelvin] and P. G. Tait — Treatise on Natural Philosophy; I (2) (2 vols.) 1879, 1883
Described as Parts 1 and 2 of Vol. I, these substantial volumes are all that appeared of the projected *Treatise*. This edition is greatly enlarged from the first, but subsequent issues are mere reprints.

J. C. Maxwell — Electricity and Magnetism (2) (2 vols.) - - - 1881

R. Potts — Euclid's Elements (2 (1861) rep.) - - - - 1865

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A collection of addresses and essays, edited by C. G. Knott.

Handbook of the Exhibition of Napier relics and of books, instruments, and devices for facilitating calculation - - - - - - 1914
Edited by E. M. Horsburgh. Articles and illustrations make this book of permanent interest in spite of its primary purpose, and a bound edition was issued by Bell under the title *Modern Instruments of Calculation.*

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* *These copies belonged at one time to T. P. Kirkman, and have marginal notes in his shorthand.*

Two books have been given in exchange for duplicates:

Lord Kelvin — Mathematical and Physical Papers; I - - - - 1882
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J. W. Russell — Pure Geometry - - - - - - - - 1893
Replacing a copy lost in Ireland in 1920.

Journal of the Mathematical Association of Japan for Secondary Education.

Thanks to the kindness of Prof. T. Hayashi, our set of this periodical lacks only one number, namely, Vol. 2, No. 1, and we should be very grateful to anyone who could supply us with the missing part. We have a few duplicates of other numbers of the *Journal* available for exchange.

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Œuvres de Huygens. Edition définitive.

Prof. Genese has sent us Vol. 8 of this edition. Sad to say, this is one of the volumes already here, so the Librarian would be glad to exchange it for one of the volumes still absent; the volumes we have are 1, 4, 8-11, 14.

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Mélanges de Mathématiques et de Physique. By EMILE PICARD. Pp. 364. 25 frs. 1924. (Gauthier-Villars.)

Statics, including Hydrostatics and the Elements of the Theory of Elasticity. 2nd edn. By H. LAMB. Pp. xii+357. 12s. 6d. net. 1924. (Cam. Univ. Press.)

Scientific Papers, mainly on Electrodynamics and Natural Radiation. By the late S. B. M'LAREN. Prepared for publication by H. H. HASSÉ, T. H. HAVELOCK, J. W. NICHOLSON, and Sir JOSEPH LARMOR. PP. vi+112. 8s. 6d. net. 1925. (Cam. Univ. Press.)

Aufgaben und Lehrsätze aus der Analysis. Vol. I. *Reihen. Integralrechnung. Funkionentheorie.* By G. PÓLYA and G. SZEGÖ. 15 m. 1925. (Springer, Berlin.)

An Introduction to Mathematical Probability. By J. L. COOLIDGE. Pp. xii+216. 15s. net. 1925. (Oxford Univ. Press.)

How to Calculate Correlations. By G. H. THOMSON. Pp. 24. 2s. net. 1925. (Harrap.)

Binomial Factorisations, giving Extension Congruence-Tables and Factorisation Tables. Vol. III. By A. J. C. CUNNINGHAM. Pp. A to D+lxix+203.

Nouvelles Vues Faraday-Maxwelliennes (Supplément). Sur la Propagation de la Lumière. By L. R. E. MENGES. Pp. 44. 5 fr. 1925. (Gauthier-Villars.)

The Theory of Relativity. 2nd edition, enlarged. By L. SILBERSTEIN. Pp. x+563. 25s. net. (Macmillan.)

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An Eighth List of Writings on Determinants. Pp. 165-184. SIR T. MUIR. Reprint from *Quarterly Journal*, I. No. 196. 1924.

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Algebraic Geometry. A First Course, including an Introduction to the Conic Sections. By M. P. MESHENBERG. Pp. xi+127. 3s. 6d. net. 1924. (Sidgwick & Jackson.)

Elementarmathematik vom Höheren Standpunkte aus. I. 3rd edit. Pp. xii+322. 15 gold marks. 1924. (Springer, Berlin.) Arithmetik—Algebra—Analysis.

Leçons de Géométric Elémentaire. By E. A. FOUËT. Pp. xvi+348. 12 fr. 1924. (Vuibert, Paris.)

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Graduated Problem Papers. By R. M. WRIGHT. Pp. viii+88. 5s. 6d. 1924. (Cam. Univ. Press.)

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A Suggestion for the Development of the Ordinary Differential Operator. By J. BRILL. Reprint. Pp. 355-8. Proc. London Math. Soc. II. 23. Part 5.

Abhandlungen aus dem Mathematischen Seminar der Hamburgischen Universität. (Math. Seminars, Hamburg.)

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American Journal of Mathematics. (Johns Hopkins Press, Baltimore.)

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Annals of Mathematics. (Princeton Univ. Press.)

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Publications de la Faculté des Sciences de l'Université Masaryk.

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Revista Matemática Hispano-Americana. (Soc. Mat. Española, Madrid.)

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Revue Semestrielle des Publications Mathématiques. *Tables des Matières.* Vols. XXVI-XXX. (1819-1923.) Pp. 149. 1925. (Gauthier-Villars.)

School Science and Mathematics. (Mount Morris, Illinois.)

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